THE

ASTROPHYSICAL JOURNAL

AN INTERNATIONAL REVIEW OF SPECTROSCOPY
AND ASTRONOMICAL PHYSICS

VOLUME IV OCTOBER 1896 NUMBER 3

THE EFFECT OF PRESSURE ON WAVE-LENGTH.

By J. F. MOHLER.

IN an article[1] published in this JOURNAL (February 1896) it was shown that pressure on the arc produced a change in the wave-length of the light emitted. The results showed that in the range of pressures between one and fourteen and a half atmospheres the shift of the lines was proportional to the pressure above one atmosphere, and that it was always toward the red end of the spectrum; and also that for any one element the shift was generally proportional to wave-length.

The work has been continued to find the effect of lower pressures and also to study the effect of different methods of producing the spectra. The spectroscope, pressure cylinder, dividing engine and micrometer eyepiece used in the former investigation were also used in this work. It is but just to say that the pressure cylinder used and described in this JOURNAL (2, 116) was designed and constructed by Professor Rowland several years ago for investigating the effect of pressure on the spectra of the elements, but laid aside until he could find time for the experiment. This cylinder was fitted with a mercury

[1] W. J. HUMPHREYS and J. F. MOHLER: "Effect of Pressure on the Wave-length of Lines in the Arc-spectra of Certain Elements."

gauge and a good air pump served to reduce the pressure. The pump was kept working slowly while the exposure was being made and it was easy to keep the pressure less than 2^{cm} of mercury. Almost all the measurements were made on photographic plates in the manner described in the former work. For a part of the work with cadmium a form of vacuum tube kindly furnished by Professor Michelson was used. It was the same form that he used in his determination of the length of the meter[1] in terms of the wave-length of light. An ordinary "end on" vacuum tube was also used. It contained a few centigrams of cadmium and the air was exhausted to 2^{mm} pressure. As the temperature to which the tube had to be raised to volatilize the cadmium was nearly 300° C. this pressure was about doubled when the tube was in use. Six storage cells and a Ruhmkorff coil giving a two-inch spark were used with these tubes. Most of the work was done at night, when the Physical Laboratory is comparatively free from vibration, and when consequently one can obtain better definition than at other times. For the arc light used in this night work a storage battery giving 85 volts and a low amperage was used.

CADMIUM.

Particular attention was given to cadmium, in the hope that the discrepancy between Professor Rowland's[2] Table of Standard Wave-lengths and Professor Michelson's measurements made at Paris might be explained. The wave-lengths of the red, green, and blue cadmium lines as given by Professor Michelson's measurements of the Standard Meter are 6438.472, 5085.824 and 4799.911 Ångström units; and the values for the same lines from Professor Rowland's tables[1] are 6438.680, 5086.001, and 4800.097, giving differences of .208, .177, and .186. These values are for the wave-length in air at the standard pressure, 760^{mm} of mercury and at 16° Centigrade. These wave-lengths

[1] "Détermination expérimental de le valeur du mètre en longeurs d'ondes lumineuses."

[2] *A. and A.*, **12**, 1893.

reduced to a vacuum would be 6440.262, 5087.238, and 4801.245 for Professor Michelson's measurements and 6440.470, 5087.415, and 4801.431 for Professor Rowland's values, giving the same differences as above.

A redetermination of the green cadmium line in the arc compared with the standard lines in the Sun[1] at wave-lengths 5014.422, 5015.123, 5020.208, 5022.414, 5068.944, 5074.932, 5083.518, 5105.718, 5109.827, and 5110.574 gave a result of 5086.000, which agrees with the above value.

Now if the above difference in the values of the wave-length of the green cadmium line is due to differences in the standards of length used, the other differences ought to be proportional to the wave-length, *i. e.*, for the blue line it ought to be .167 and for the red line .223, making an error of +.019 in one case and −.015 in the other: such an error as would be caused by a difference of barometric pressure of one centimeter of mercury or the expansion of the standard of length by a change of one degree Centigrade. This effect of pressure on wave-length is simply due to the fact that the index of refraction of air changes with the pressure and the wave-frequency is not changed at all. The change of wave-length noted below is of an entirely different character, and indicates a change in the vibration frequency of the waves.

The effect of decreasing the pressure on the arc producing the lines is to shift them slightly toward the violet end of the spectrum. The amount of this shift is about the same per atmosphere as it was for higher pressures, so that it could be stated that the shift of the lines varies directly as the pressure, and the standard wave-lengths would naturally be referred to a vacuum. In the former work the average *measured* shift of the green and blue cadmium lines was .010 Ångström unit toward the red for each additional atmosphere pressure. With the arc under a pressure of two or three centimeters of mercury the shift of these two lines on six different plates was .014 ± 3 toward the violet. These values were obtained by comparing the lines

[1] Tables by ROWLAND now being published in the ASTROPHYSICAL JOURNAL.

produced by the arc at 2^{cm} mercury pressure with those produced by the arc at $76^{cm}.36$ pressure, and also by comparing the lines from the arc at low pressure with the standard lines in the Sun given above.

Mr. L. E. Jewell made one set of measurements with the eyepiece micrometer on the red cadmium line, comparing it with the solar standards at wave-lengths 6431.063, 6439.298, and 6450.029, and found the shift when the pressure was removed to be .010 Ångström unit toward the violet.

With vacuum tubes the lines obtained were faint from under exposure and not good for very accurate measurement, and both tubes broke after the first heating. The plates obtained, however, showed that the position of the lines at 2^{mm} pressure produced by the spark was the same as that of the lines produced by the arc under a pressure of 2^{cm} of mercury. Professor Michelson's method of producing the light gives a much *sharper* line than can be obtained with the arc.

The shift of the ultra-violet line at wave-length 3403.77, however, was much smaller than that of the three other cadmium lines referred to above. It was a good line on all the plates, and the measured shift was about .004 Ångström unit. On looking up my original notes on the previous work at higher pressures I find that here also the shift of this ultra-violet line is much smaller than that of the lines in the visible part of the spectrum, and that the difference is much greater than would be found if the shift were proportional to wave-length. Other ultra-violet lines that occur in the cadmium series[1] were found to have a very small shift. A difference of the same kind is found in the magnesium lines and suggests a method for identifying lines of a series in an element.

MERCURY.

In the previous paper it was shown that for many elements the shift of the lines is approximately inversely proportional to the absolute temperature of the melting point. If this were

[1] J. S. AMES: *Phil. Mag.* (5), **30**, 1890.

true of all the elements, then mercury would have a very large shift. Several attempts to get the shift of the lines of mercury under pressure were made but the lines tried spread out too much to admit of accurate measurement. There is, however, a group of lines at wave-lengths 3650.31, 3654.94 and 3663.25[1] which are sharp when there is little material in the arc and are nicely reversed when more material is present. Ten plates of this region were taken, several under a pressure of five to six atmospheres and the others at as low a pressure as could be obtained with the air pump. These plates show that for the arc at low and high pressure the shift is proportional to pressure The shift at pressures from 2^{cm} to 3^{cm} of mercury was $.005 \pm 2$ Ångström unit toward the violet, the position of the lines at one atmosphere pressure being taken as standard. The shift at $5\frac{1}{2}$ atmospheres pressure was $.020 \pm 3$ Ångström unit toward the red, and at $6\frac{1}{4}$ atmospheres pressure the shift was $.022 \pm 3$ Ångström unit.

These lines appear very sharp and clear when a spark is passed through a vacuum tube that has been exhausted by a mercury pump. Measurements of these lines on several plates taken by Dr. Ames and also on one taken by myself give results so discordant with each other and with the results given above for the arc that further work with this element will be necessary before it can be affirmed that the arc and spark produce lines of the same wave-length in a vacuum.

OTHER ELEMENTS.

The shift of the lines of many of the elements for one atmosphere pressure is such a small quantity that the measurement was attempted only when the shift was comparatively large or the lines numerous and well defined. Iron was represented on nearly all the plates by sharp lines due to impurities in the carbons, and these were always measured. The average of all these measurements of the iron lines gives a shift of .002 Ångström unit toward the violet, agreeing with the former work.

[1] KAYSER and RUNGE, *Abh. d. K. Akad. d. W. Berlin*, 1891–3.

Indium gave us the largest measured shift under high pressure and several plates were taken of the indium lines at wave-lengths 4102.000 and 4511.345 at low pressure. Measurements on these lines showed a shift of .010 ± 4 Ångström unit toward the violet. There was one precaution, however, that it was absolutely necessary to take, which was to have the lines of about the same intensity for comparison. A heavy line at low pressure, unless it was reversed, might appear to coincide exactly with a fine line at one atmosphere pressure. In this case the unsymmetrical widening of the line was sufficient to counteract the shift due to change in pressure. This source of error was more noticeable with small pressures than with large. In general, however, the lines under low pressure were very much sharper than under heavy pressure, so that though the shift to be measured was much smaller the accuracy of setting was greater.

The shift of the zinc lines at wave-lengths 4722.342 and 4810.724 was .007 ± 3 Ångström unit toward the violet, and for the line at wave-length 3018.5 the shift was .004 ± 2 Ångström unit, agreeing well with the former work. These lines are also in the regular series of zinc triplets.

The shift of the magnesium lines of the *b* group at wave-lengths 5167.488, 5172.866 and 5183.791 was .010 ± 2 Ångström unit toward the violet; while the shift of the symmetrical group of fine ultra-violet lines at wave-lengths 2776.798, 2778.381, 2779.935, 2781.521 and 2783.077 and the pair at wave-lengths 2795.632 and 2802.805 was .006 ± 3 Ångström unit toward the violet. The shift of the strong line at wave-length 2852.239 was considerably larger, but owing to the width of the reversal could not be very accurately measured.

An attempt was made to find the shift of the compound thallium line at wave-length 5350.670, but though unsuccessful in determining the shift, it may serve to show how some compound lines may have a large *apparent* shift. The line in question is a triplet. At one atmosphere pressure with but little of the element in the arc the violet component is much the strongest and the

line in the middle is the weakest. At very low pressure the red component is the strongest, and the violet component the weakest. The two thallium lines at wave-lengths 3519.342 and 3529.547 show but a small displacement with pressure.

The general effect of pressure, when the pressure was due to air, on the carbon bands was to greatly increase their strength and prominence. At high pressures the carbon bands would be much stronger than the metallic lines. At low pressures, however, they were much fainter than at atmospheric pressure and far less prominent compared with the metallic lines. The single carbon line at wave-length 2478.661, however, showed as plainly under reduced pressure as at atmospheric pressure.

In the above results the $\pm$ indicates not the calculated probable error but the actual variation in results from different plates.

I wish here to acknowledge my indebtedness to Professor Rowland and Dr. Ames, the directors of the Physical Laboratory, for direction and assistance in the investigation; also to Professor Michelson for the vacuum tube sent me, and to Mr. L. E. Jewell for assistance in the work.

JOHNS HOPKINS UNIVERSITY,
June 1896.

SOLAR OBSERVATIONS MADE AT THE ROYAL OBSERVATORY OF THE ROMAN COLLEGE DURING THE FIRST HALF OF 1896.

By P. TACCHINI.

I TAKE pleasure in sending you a résumé of the solar observations made at the Royal Observatory of the Roman College during the first half of 1896. The weather has been exceptionally favorable. Following are the results for the spots and faculæ:

1896	Number of days of observation	Relative Frequency		Relative Size		Number of groups of spots per day
		of spots	of days without spots	of spots	of faculæ	
January	25	5.48	0.00	30.0	60.2	2.5
February	24	12.79	0.00	51.9	64.8	3.3
March	24	10.75	0.00	39.2	78.8	3.0
April	23	10.87	0.17	34.3	77.4	2.7
May	24	5.17	0.17	16.4	59.4	1.5
June	28	13.03	0.00	52.9	52.0	2.6

In comparing these results with those obtained for the six months preceding it is seen that the spots have continued to decrease, with a well-marked secondary minimum in May, while for the faculæ the difference is small.

For the prominences we have obtained the following results:

1896	Number of days of observation	Prominences		
		Mean number	Mean height	Mean extent
January	23	5.22	40".4	2°.3
February	24	5.79	41 .7	2 .0
March	16	4.56	34 .0	1 .3
April	23	3.26	33 .0	1 .5
May	24	4.06	36 .7	1 .8
June	24	4.67	37 .3	1 .6

Thus the prominences have also shown a diminution, with a secondary minimum in April and May.

The following table contains the results which I have obtained for the distribution in latitude of solar phenomena during this period:

1896	Prominences				Faculæ				Spots			
Latitude	First Quarter		Second Quarter		First Quarter		Second Quarter		First Quarter		Second Quarter	
90° +80°	0.006		0.007									
80 +70	0.003		0.000									
70 +60	0.006		0.003		0.000		0.004					
60 +50	0.006		0.036		0.000		0.004					
50 +40	0.052	0.495	0.085	0.501	0.000		0.000					
40 +30	0.114		0.134		0.016	0.436	0.009	0.328				
30 +20	0.136		0.103		0.108		0.079		0.000		0.000	
20 +10	0.114		0.087		0.172		0.123		0.250	0.391	0.245	0.388
10 + 0	0.058		0.046		0.140		0.109		0.141		0.143	
0° −10°	0.086		0.036		0.151		0.157		0.047		0.163	
10 −20	0.130		0.101		0.236		0.267		0.421	0.609	0.347	0.612
20 −30	0.116		0.092		0.145		0.205		0.141		0.102	
30 −40	0.097		0.098		0.027	0.564	0.035	0.672				
40 −50	0.050	0.505	0.105	0.499	0.005		0.004					
50 −60	0.014		0.043		0.000		0.004					
60 −70	0.006		0.007		0.000		0.000					
70 −80	0.006		0.010									
80 −90	0.000		0.007									

The frequency of the prominences by zones was the same in both hemispheres of the Sun. The prominences were always rather numerous from the equator to $\pm 50°$, as in the preceding six months, while they were rare or altogether absent in higher latitudes. The faculæ have been most frequent in the southern zones, and always numerous from the equator to $\pm 30°$, with their maximum frequency in the zones $(\pm 10° \pm 20°)$.

The spots, like the faculæ, show a greater frequency in the southern hemisphere, the maximum being also in the zones $(\pm 10° \pm 20°)$, while the maxima for the prominences occur in much higher latitudes.

Only one eruption was observed: on February 5, in the southern hemisphere at $-14°.3$ W.

ROME, August 1895.

CERTAIN CONSIDERATIONS CONCERNING THE ACCURACY OF EYE-ESTIMATES OF MAGNITUDE BY THE METHOD OF SEQUENCES.

By ALEXANDER W. ROBERTS.

IN an article on the light curve of certain variable stars obtained photometrically, Professor Pickering makes the following averments (ASTROPHYSICAL JOURNAL, **3**, 281):

(1) When repeated observations have to be made on the same star, during the same evening, it is impossible, by the method of sequences, to obtain independent estimates.

(2) It is impossible to assign absolute values to visual determinations of magnitude.

(3) In any careful study of the causes of the variation of different stars, photometrical measurements are almost indispensable.

Put in other, and more direct language, these statements amount to this: that determinations of stellar variation by Argelander's method are usually inaccurate, generally uncertain, and always insufficient for important lines of investigation. These, if sustained, are serious charges against a method of observation that hitherto has been so zealously followed, and followed with no mean measure of success. It is unfortunate, however, that Professor Pickering should have preferred his arraignment of visual determinations of magnitude without the usual accompaniment of proof that might be accepted or rebutted, and as one cannot accept a simple *ipse dixit* even when stated by an astronomer of Professor Pickering's reputation and authority, in a matter of such importance as the question that has been raised, I think it is only due to those who, year in year out, labor to accumulate data for future discussion concerning the nature and cause of stellar variation, that such proof should be forthcoming without delay.

Meanwhile I would, from my own experience in variable star

work, instance certain facts which seem to point to a different conclusion from that reached by Professor Pickering. It is to be noticed that the first statement is of more serious import than the remaining two, as it deals directly with the value of eye-estimates in determining light curves of variables of the Algol type, or of the type of U Pegasi and S Antliæ. Recently another variable of this new type has been discovered at Lovedale, and as my observations of this star supply important testimony to the reliability and accuracy of eye-estimates of magnitudes, even when a long series of measures are made on the same evening, I may be pardoned for referring to them. I am conscious that much stronger evidence could have been adduced from the observations of other variable star observers. The extreme accuracy both as to limits of variation, and times of maxima and minima, secured by Dr. Chandler from his observations of U Pegasi[1], when terms are introduced for the elimination of position error, is an instance in point. I am more conversant, however, with my own work, and I think the mode of eliminating "position error" adopted here, raises somewhat the accuracy of the observations made.

The star to which I refer is L 5861, period $7^h\ 16^m$. The variation of this star was suspected for more than two years. In May of the present year I began systematic observations of it, the method adopted being to begin observations soon after sunset and to continue the observations at intervals of twenty minutes or half an hour, for about six or eight hours. It soon became evident that the period of the star could not be more than eight or nine hours; still the amplitude variation was so narrow, only $0^m.4$, that I determined to make no definitive discussion of the observations until I had secured a large number of them. During May, June and July, I regularly observed the star in the manner described, using no ephemeris, and with complete ignorance as to what period or type of variation the observations would yield. In July the observations were reduced, the form of the light curve being determined by a least-

[1] *Ast. Jour.*, No. 374, p. 107.

square solution of the observations, that is the observations on any evening were made to yield equations of condition of the form,

$$x + \alpha \cos t + \beta \sin t + \gamma \cos 2t + \delta \sin 2t = 0,$$

and the residuals, $o - c$, were computed rigorously from this expression.

The mean discordance of a single observation was found to be $\pm 0^m.037$.

Are we to understand that this discordance is not absolute, that it is vitiated by some unconscious obscuring of the judgment consequent upon an unconscious mental solution of over 400 equations of condition, involving seven unknown quantities, by the unconscious construction of an ephemeris from the elements obtained in this unconscious mental jugglery? Such a conclusion would be preposterous. This mean discordance of $\pm 0^m.037$ represents the actual face value of visual determinations of magnitude, when the method of double observations is strictly adhered to.

It is quite possible that mental bias may have influenced this result, but I would desire it to be noted that the conditions of observing were such that any bias, or predetermination of maxima and minima points, would increase the true average discordance, and thus the resulting average error $\pm 0^m.037$ *includes* any effects arising from this source.

I think this will be readily admitted when a detailed series of observations is given, just as they were secured. And in this connection, I would desire to point out that the varying positions of the variable and the comparison stars constantly tend to produce change in the apparent magnitudes, and thus the validity of Professor Pickering's contention, that the similarity of each succeeding observation lessens independent judgment, is strongly questioned. These arguments will be better exemplified by taking one night's observations of L 5861. (See Table (1), page 187.)

OBSERVATIONS OF L 5861 MADE ON JULY 1, 1896.

(1) TAKEN WITH A REVERSING EYEPIECE.

Time	Comparison Stars					Var. L 5861
	A. Z. C. xiv. 505	A. Z. C. xiv. 639	A. Z. C. xiv. 837	A. Z. C. xiv. 259	A. Z. C. xiv. 512	
6h 0 m	7m.2	7m.7	8m.0	7m.1	7m.9	7m.7
30	7 .0	7 .6	7 .9	7 .3	7 .8	7 .6
7 0	7 .0	7 .8	8 .0	7 .4	7 .9	7 .6
10	6 .9	7 .8	7 .9	7 .6	7 .9	7 .5
25	6 .9	7 .8	7 .7	7 .6	7 .9	7 .4
45	6 .9	7 .9	7 .8	7 .6	7 .9	7 .4
8 0	6 .9	7 .8	7 .7	7 .5	7 .6	7 .3
15	6 .9	7 .8	7 .6	7 .7	7 .9	7 .3
30	6 .9	7 .9	7 .6	7 .6	7 .9	7 .3
50	6 .9	7 .9	7 .6	7 .7	7 .9	7 .3
9 10	6 .9	7 .9	7 .6	7 .6	7 .9	7 .3
30	6 .9	7 .9	7 .6	7 .7	7 .9	7 .35
50	6 .9	7 .9	7 .6	7 .7	7 .9	7 .35
10 15	6 .9	7 .8	7 .6	7 .7	7 .9	7 .4
33	6 .9	7 .6	7 .3	7 .9	7 .9	7 .6
50	6 .9	7 .6	7 .4	7 .9	7 .9	7 .6
11 15	6 .9	7 .6	7 .3	8 .0	7 .9	7 .7
30	7 .0	7 .6	7 .2	8 .0	7 .9	7 .7
12 0	7 .0	7 .4	7 .0	7 .9	7 .9	7 .7
30	7 .0	7 .4	7 .0	7 .9	7 .9	7 .7
50	7 .2	7 .4	7 .0	8 .0	8 .0	7 .8
13 20	7 .4	7 .4	7 .0	8 .0	8 .0	7 .7
45	7 .6	7 .4	7 .0	8 .0	8 .0	7 .8

If now the successive measures of the comparison stars be examined, the influence of position on the apparent magnitude will be clearly evident, and my contention that there is no danger of the mind being influenced by preceding observations abundantly proved. Indeed, if we had no other record except the above, we would naturally conclude that every comparison star used is also a variable star. The above series is, as stated, taken with an ordinary reversing eyepiece.

Let now the observations taken at the same time with a direct-vision eyepiece be examined.[1] (Table (2), page 188.)

[1] The time of observation given in both is the mean time between the two observations.

(2) TAKEN WITH A DIRECT-VISION EYEPIECE.

Time	Comparison Stars. A. Z. C. xiv. 505	A. Z. C. xiv. 639	A. Z. C. xiv. 837	A. Z. C. xiv. 259	A. Z. C. xiv. 512	Var. L 5861
6^h 0 m	$7^m.4$	$7^m.2$	$7^m.0$	$7^m.9$	$7^m.7$	$7^m.8$
30	7 .4	7 .2	7 .0	7 .9	7 .8	7 .7
7 0	7 .6	7 .1	7 .0	7 .8	7 .8	7 .8
10	7 .6	7 .0	7 .2	7 .9	7 .8	7 .8
25	7 .6	7 .0	7 .3	8 .0	7 .9	7 .7
45	7 .6	7 .0	7 .3	7 .8	7 .9	7 .6
8 0	7 .6	7 .0	7 .3	7 .7	7 .8	7 .6
15	7 .6	7 .0	7 .4	7 .5	7 .8	7 .6
30	7 .6	7 .0	7 .4	7 .6	7 .7	7 .5
50	7 .7	7 .0	7 .4	7 .4	7 .9	7 .6
9 10	7 .7	7 .0	7 .5	7 .5	7 .9	7 .6
30	7 .7	7 .0	7 .6	7 .5	7 .8	7 .6
50	7 .7	7 .0	7 .6	7 .5	7 .9	7 .6
10 15	7 .6	7 .0	7 .6	7 .5	7 .8	7 .7
33	7 .6	7 .0	8 .0	7 .4	7 .8	7 .8
50	7 .6	7 .2	7 .9	7 .4	7 .8	7 .7
11 15	7 .6	7 .3	7 .9	7 .0	7 .7	7 .8
30	7 .6	7 .3	7 .9	7 .0	7 .7	7 .8
12 0	7 .6	7 .3	8 .0	7 .1	7 .8	8 .0
30	7 .6	7 .4	8 .0	7 .2	7 .8	8 .0
50	7 .4	7 .4	8 .0	7 .0	7 .8	8 .0
13 20	7 .4	7 .5	8 .0	7 .0	7 .8	7 .8
45	7 .0	7 .7	8 .0	7 .1	7 .8	7 .6

An examination of the above series of measures indicates the operation of a systematic error similar to that which operates in Table (1). Further there is apparently no connection whatever between the measures in Table (1) and those in Table (2); and thus the independence of the various observations which Professor Pickering regards, and rightly regards, as the most valuable characteristic of photometric measurements, is also a feature of the method adopted at Lovedale. And I am convinced that it is this very independence which is the chief factor in reducing the mean error of a single combined observation below $\pm$ 0.^{m}04. Further, the insecurity of measures, based solely on estimates in one position only, is abundantly manifest.

Combining, now, the apparently irreconcilable observations in Tables (1) and (2) we obtain the combined measures shown in Table (3).

(3) COMBINED MEASURES.

Time	Comparison Stars. A. Z. C. xiv. 505	A. Z. C. xiv. 639	A. Z. C. xiv. 837	A. Z. C. xiv. 259	A. Z. C. xiv. 512	Total	Var. L 5861
6h .0 m	7m .30	7m .45	7m .50	7m .50	7m .80	37m .55	7m .75
.30	7 .20	7 .40	7 .45	7 .60	7 .80	.45	7 .65
7 .0	7 .30	7 .45	7 .50	7 .60	7 .85	.70	7 .70
.10	7 .25	7 .40	7 .55	7 .75	7 .85	.80	7 .65
.25	7 .25	7 .40	7 .50	7 .80	7 .90	.85	7 .55
.45	7 .25	7 .45	7 .55	7 .70	7 .90	.85	7 .50
8 .0	7 .25	7 .40	7 .50	7 .60	7 .70	.45	7 .45
.15	7 .25	7 .40	7 .50	7 .60	7 .85	.60	7 .45
.30	7 .25	7 .45	7 .50	7 .60	7 .80	.60	7 .40
.50	7 .30	7 .45	7 .50	7 .55	7 .90	.70	7 .45
9 .10	7 .30	7 .45	7 .55	7 .55	7 .90	.75	7 .45
.30	7 .30	7 .45	7 .60	7 .60	7 .85	.80	7 .48
.50	7 .30	7 .45	7 .60	7 .60	7 .90	.85	7 .48
10 .15	7 .25	7 .40	7 .60	7 .60	7 .85	.70	7 .55
.33	7 .25	7 .30	7 .65	7 .65	7 85	.70	7 .70
.50	7 .25	7 .40	7 .65	7 .65	7 .85	.80	7 .65
11 .15	7 .25	7 .45	7 .60	7 .50	7 .80	.60	7 .75
.30	7 .30	7 .45	7 .55	7 .50	7 .80	.60	7 .75
12 .0	7 .30	7 .35	7 .50	7 .50	7 .85	.50	7 .85
.30	7 .30	7 .40	7 .50	7 .55	7 .85	.60	7 .85
.50	7 .30	7 .40	7 .50	7 .50	7 .90	.60	7 .90
13 .20	7 .40	7 .45	7 .50	7 .50	7 .90	.75	7 .75
.45	7 .30	7 .55	7 .50	7 .55	7 .90	37 .80	7 .70
Mean Er. ±	0m.031	0m.036	0m.046	0m.051	0m .039		

The residuals in the last line of this table accordingly represent absolute mean discordances, inasmuch as each observation is always related to the standard magnitude $6^m.8$ — the limit of vision —, and there are some stars near that are just visible and no more.

One more step is taken in the discussion of a series of observations, viz., the observations are reduced to a common standard by making the totals similar, and the same correction is applied to the variable as is applied to each of the comparison stars. (See Table (4), page 190.)

The computed values in 9th column of Table (4) are calculated from the expression

$$7^m.646 - 0^m.223 \cos(\theta - 357^\circ\ 37') - 0^m.014 (2\theta - 156^\circ\ 44')$$

where $\theta = (T - 8^h\ 50^m)\ 0.^\circ 8257$.

(4) REDUCED OBSERVATIONS.

Time	Comparison Stars					Total	Var.	Var. Comp.	O.—C.
	A. Z. C. xiv. 505	A. Z. C. xiv. 639	A. Z. C. xiv. 837	A. Z. C. xiv. 259	A. Z. C. xiv. 512				
6^h 0^m	$7^m.33$	$7^m.48$	$7^m.53$	$7^m.53$	$7^m.83$	37.70	$7^m.78$	$7^m.81$	$-0^m.03$
30	7 .25	7 .45	7 .50	7 .65	7 .85	.70	7 .70	7 .72	— .02
7 0	7 .30	7 .45	7 .50	7 .60	7 .85	.70	7 .70	7 .62	+ .08
10	7 .23	7 .38	7 .53	7 .73	7 .83	.70	7 .63	7 .60	+ .03
25	7 .22	7 .37	7 .47	7 .77	7 .87	.70	7 .52	7 .56	— .04
45	7 .22	7 .42	7 .52	7 .67	7 .87	.70	7 .47	7 .51	— .04
8 0	7 .30	7 .45	7 .55	7 .65	7 .75	.70	7 .50	7 .48	+ .02
15	7 .27	7 .42	7 .52	7 .62	7 .87	.70	7 .47	7 .46	+ .01
30	7 .27	7 .47	7 .52	7 .62	7 .82	.70	7 .42	7 .44	— .02
50	7 .30	7 .45	7 .50	7 .55	7 .90	.70	7 .45	7 .44	+ .01
9 10	7 .34	7 .44	7 .49	7 .54	7 .89	.70	7 .44	7 .44	+ .00
30	7 .28	7 .43	7 .58	7 .58	7 .83	.70	7 .46	7 .47	— .01
50	7 .27	7 .42	7 .57	7 .57	7 .87	.70	7 .45	7 .50	— .05
10 15	7 .25	7 .40	7 .60	7 .60	7 .85	.70	7 .55	7 .56	— .01
33	7 .25	7 .30	7 .65	7 .65	7 .85	.70	7 .70	7 .62	+ .08
50	7 .23	7 .38	7 .63	7 .63	7 .83	.70	7 .63	7 .68	— .05
11 15	7 .27	7 .47	7 .62	7 .52	7 .82	.70	7 .77	7 .76	+ .01
30	7 .32	7 .47	7 .57	7 .52	7 .82	.70	7 .77	7 .80	— .03
12 0	7 .34	7 .39	7 .54	7 .54	7 .89	.70	7 .89	7 .87	+ .02
30	7 .32	7 .42	7 .52	7 .57	7 .87	.70	7 .87	7 .88	— .01
50	7 .32	7 .42	7 .52	7 .52	7 .92	.70	7 .92	7 .86	+ .06
13 20	7 .39	7 .44	7 .49	7 .49	7 .89	.70	7 .74	7 .80	— .06
45	7 .28	7 .53	7 .48	7 .53	7 .88	37.70	7 .68	7 .72	— .04
Mean Er.±	$0^m.036$	$0^m.034$	$0^m.036$	$0^m.059$	$0^m.030$				$0^m.032$

I have given in full detail the observations of L 5861 taken on one night, because the only arguments for or against the oft-repeated depreciation of eye-estimates worth considering are facts, not opinions.

The observations given, those of July 1, 1896, are in no way outstanding. I have only referred to them here because in another paper dealing with the variation of L 5861 I have selected this night's observation as a good instance of how the mean curve is obtained. On some other nights in July the accordance between the several measures is even more striking.

Is it possible that these measures can be vitiated by mental bias? Not only does the ever changing hour angle change the relative magnitudes, when viewed with the same eyepiece, but

each observation with the reversing eyepiece is immediately followed by an observation with a direct vision eyepiece, which completely alters the apparent magnitude of every star under consideration. Each set is different from those that precede and those that follow; similarity in the estimates means error. Then again, between each set of two, four or five observations of other variables are taken; thus an open mind encounters each observation in turn, and complete mental detachment is secured.

As regards bias, accordingly, measurements by Argelander's method can be made as secure as any series obtained photometrically.

The mean error obtained from an application of a common light curve to all the observations of L 5861, viz.,

$$\pm 0^m.037$$

is in no way singular.

In the *Astronomical Journal*, No. 327, I have given in detail the observations taken during 1894 of the southern Algol variable, S Velorum.

The average discordance between the observed places and a mean curve computed rigorously from the expression,

$$M = x + at + \beta t^2 + \gamma t^3 + \delta t^4$$

is for the different nights:

1894	April	1	$\pm 0^m.042$	(*Ast. Jour.* No. 327, p. 115)
	April	13	.036	
	May	1	.032	
	May	7	.044	
	May	25	.042	
	May	31	.056	Weight ¼
	July	23	.037	
	July	29	± 0 .015	Weight ¼
Average error			± 0 .038	

The form of the light curve obtained for this variable not only corresponds with that obtained photographically, but it is in strict harmony with the curve obtained from a geometrical consideration of the eclipse of one star by another.

In the *Astronomical Journal*, No. 373, are given in detail the observations secured here of R. S. Sagittarii.

The mean discordance which follows from the application of a single light curve to all the measures is $\pm 0^m.036$. And here I may point out that this constant error is also that of the determinations of the magnitudes of the comparison stars used, when reduced to a common standard.

Observations of these three stars have already been published because of their immediate importance; an examination, however, of over 12,000 observations (chiefly of short period variables) not yet published, yields almost identical results. The mean error of sixteen new southern short period variables is

$$\pm 0^m.040.$$

Is this constant mean error to be considered entirely fortuitous? Such a contention is out of the question.

This constant value $\pm 0^m.04$ is the measure of the accuracy of this method of determining stellar magnitudes; and all other claims to a more consistent gauge, or a more rigorous standard of measurement, or even more trustworthy results, must be related to this value. No other conclusion is tenable or possible.

I take it, therefore, that Professor Pickering's first averment is something more than an unproved statement; it is an unprovable one.

We are naturally led from a consideration of the first statement to the second, viz., that it is impossible to assign absolute values to visual determinations of magnitude. This statement has so often been made, that at last it has become somewhat axiomatic, and this notwithstanding—

(1) That there is more conformity between so-called arbitrary magnitudes determined visually, than between magnitudes determined photometrically. An examination of different uranometria will abundantly prove this.

(2) That photometrical determinations of magnitude are also arbitrary, inasmuch as if we consider the typical relation between photometrical magnitudes, viz.,

$$\frac{L_x}{L_{x+n}} = K^n$$

x and K must be arbitrary.

(3) The natural relation between magnitudes is the visual one, inasmuch as it is the relation which appeals directly to the senses without any intermediate relationship.

(4) The visual standards of reference $6^m.8$ and $9^m.3$ are clearly defined points of reference.

The question however is not an important one in its relation to the physical causes which underlie stellar variation. The same objection, also, could be as reasonably urged against the use of the heliometer for the determination of planetary positions. Like the heliometer, eye-estimates are differential in their principle of operation; but all differential measures may be made absolute by linking them on to one or more positions absolutely determined. In eye-estimates we have at least two such well-defined standard values, $6^m.8$ and $9^m.3$.

These two points have been determined again and again by different observers. They accordingly form the terminal points of our photometric base line. Between these two points a series of measures can be obtained, not one of which will be $0^m.1$ in error. Indeed, if observations be repeated over and over again, as in the case of the comparison stars for L 5861, the average error will be reduced within $0^m.05$.

We have already seen that the average *absolute* errors (Table (3)) for the comparison stars are

A. Z. C. xiv.	505	$= 0^m.031$
	639	$= 0\ .036$
	837	$= 0\ .046$
	259	$= 0\ .051$
	512	$= 0\ .039$

Mean absolute error $= 0^m.041$.

That we call a star just visible to the naked eye $6^m.8$ and a star just visible in a good 1^{in} glass $9^m.3$, on a clear night, at altitude 60°, because these values accord best with the conventional values of Argelander and Gould, does not make the method of

eye-estimates one based on arbitrary values, any more than the photometrical determinations of magnitude are arbitrary because $2^m.1$ is taken as the magnitude of Polaris, and 2.512 as the value of the light ratio.

The one method, in application and in results, is as absolute as the other.

"In any careful study of the causes of the variation of different stars, photometrical measurements are almost indispensable," is Professor Pickering's third contention. It is the second sentence in Professor Pickering's interesting article, but I have considered it here because what has gone before would be sufficient refutation of the statement if there were not another and more direct argument against its acceptance. The "careful study of the variation of different stars" by the "indispensable" photometric method has still to be made. All the knowledge we have of the physical conditions of the orbital movement of such stars as Algol,[1] Z Herculis, R. S. Sagittarii, and S Velorum we owe to the simple method of eye-estimates by sequences.

And it will always be so. The mean error of a single observation by eye-estimates must be, from the very nature of things, less than the mean error of a single photometric measurement. The photometric measurement is fundamentally an eye-estimate, and must of necessity be liable to every source and possibility and amount of error that influences eye-estimates; and to this must be added the peculiar errors that are incidental alone to such indirect modes of measurement as all photometrical methods are. It is true that eye-estimates, when finally reduced, give only relative differences in magnitude, and that they do not indicate in any way the amount of light, while in investigations dealing with the causes of stellar variation we must obtain the changes in the amount of light, not in the apparent magnitude. Such a transposition can be readily effected without any reference to photometrical measurements.

[1] I do not here deal with spectroscopic results. These, of course, lie outside the range of the present paper.

I have elsewhere instanced one important and simple method, viz., superposition.

In this case

$$L_x + L_{x-n} = L_{x-m}$$

yields the identity

$$1 + K^n = K^m.$$

There is another method, equally simple, by which light grades may be reduced to light ratios. We have already instanced the precision with which absolute values can be assigned to magnitudes between $6^m.8$ and $9^m.3$. Let now a series of object glasses,[1] all less than 1^{in} be selected, and with these several object glasses let the limit of vision for different apertures between $0^{in}.3$ and $1^{in}.0$ be determined from the stars whose magnitudes have already been estimated with the greatest care. An investigation of this determination will yield data by which any so-called "arbitrary" magnitudes may be reduced to actual light values.

It is unnecessary to enter into the matter further. Many determinations similar to that just referred to have been undertaken, but they have been somewhat feeble in intention, the need for refined exactness not being then very pressing. The need is now imperative, if only as a refutation of the charge of insufficiency against eye-estimates.

The preceding paper does not bear within it any imputation against the accuracy of photometrical methods of research. I am willing to accept all that its able exponent, Professor Pickering, claims for it, *per se*. This paper also is not an apology or a justification for the method of eye-estimates. It needs not the former, and the records of variable star work during the past fifty years is surely justification enough. But this paper is a protest against the recurrent statements, again and again refuted, that eye-estimates are more inaccurate and less valuable than those made by any of the photometers at present in use.

LOVEDALE, S. Africa,
July 1896.

[1] Different apertures are not as valuable as different object glasses.

ON THE LEVEL OF SUN-SPOTS.[1]

By EDWIN B. FROST.

IN spite of the large number of solar theories now awaiting scientific acceptance, our actual knowledge regarding the Sun is decidedly limited, and the mass of statistics regarding its surface that has accumulated in recent years has thus far not greatly clarified our views as to the nature, and still less as to the causes, of the solar phenomena.

I desire for a few moments your attention in considering our present knowledge as to the elevation of Sun-spots, and I will state at the outset that in the light of recent observations I shall venture to call in question the venerable theory that Sun-spots are depressions in the solar photosphere.

In speaking of levels we must proceed from some accepted plane of reference; and the most natural plane, or surface of reference, would be the solar photosphere. Here we are abruptly confronted by the theory of Schmidt, elaborated in a convenient form by Knopf,[2] according to which the photosphere is merely an optical illusion, produced by circular refraction in the Sun itself, supposed to be a globe of glowing gas without a condensed stratum. Prominences, faculæ, spots, and the granulation are explained as effects of anomalous refractions due to local changes of density somewhere within the gas ball.

This theory, worked out as it is by careful mathematical reasoning, deserves and has received respectful consideration. Nevertheless, in view of the physical improbability of Schmidt's primary assumption that in its outer portions the gaseous mass maintains its state without condensation, the physicist will feel obliged to reject the theory, which also suffers from the fundamental defect of failing to account for the solar spectrum on the accepted principles of physics. Moreover, any one who has

[1] A paper read at the meeting of the American Association for the Advancement of Science, August 1896.

[2] Jena, 1893.

with some continuousness studied the phenomena of the solar surface must affirm that he has observed realities, not illusions. The perspective effects on prominences as they pass around the limb, the motion and permanence of spots, the displacements of spectral lines on the approaching and receding limbs, and in fact all the phenomena concerned in the solar rotation, are distinctly contradictory to Schmidt's theory. In dismissing it from further consideration, however, we shall take with us the important inference that refraction within and on the Sun itself may modify in some considerable degree the phenomena we observe.

In illustrating an application of this inference we may digress slightly by citing a theory of spots and faculæ suggested by James Renton in a recent number of *Nature*,[1] received while this paper was being written. He advances the idea that a spot and the surrounding faculæ are due to an anticyclonic area (of descending currents) over the photosphere. In consequence of the convexity of the successive surfaces of equal density in the solar atmosphere, the radiation from the photosphere centrally under the anticyclonic area will be divergingly refracted so that it will appear to emanate from a greater area than is actually the case. Hence there will result a deficiency in the intensity of the radiation as compared with that from a portion of the photosphere over which the atmospheric pressure is normal and the surfaces of equal density are concentric with the photosphere. At the outer edge of the high pressure area Renton considers that the refraction would cause the rays to converge, compressing into a smaller area of the apparent disk the radiation from a ring of photospheric surface around the "spot," with consequent increase in surface intensity, thereby producing the faculæ. While observers will probably agree that refraction must be much less effective than the general absorption by the affected atmospheric area, Renton's suggestion is nevertheless important as recognizing in a rational way the possible results of refraction.

[1] *Nature*, **54**, 317, Aug. 6, 1896

Reverting, then, from our digression, we shall adhere to the view that the photosphere does exist as a condensed stratum of some stability, granulated perhaps by the friction of overlying atmospheric currents, and we shall employ it as our surface of reference.

In now considering the elevation of Sun-spots with respect to the photosphere we shall be able to avail ourselves of four different lines of evidence: (1) direct observation; (2) the solar rotation; (3) thermal absorption; (4) spectroscopic observations.

The belief that Sun-spots are depressions in the photosphere dates back to the last century, when Wilson of Glasgow asserted this to be the necessary conclusion from observed effects of perspective. Probably most observers have at some time noted cases where the penumbra *appeared* foreshortened as the spot rounded the limb. The writer has been led to reëxamine the question of the elevation of spots from the evidence of thermal absorption, and in a paper[1] before this Section at the meeting in 1892 ventured to account for certain measurements on spot radiation on the ground that they "lie in a higher stratum than the photosphere." Within the past two years a large amount of direct observational material has been adduced by assiduous observers to show the incorrectness of the Wilsonian doctrine.

At the meeting of the Royal Astronomical Society in December 1894, F. Howlett presented to the Society[2] several volumes of solar drawings which in all represented the devotion of thirty-five years to the study of the Sun's surface, chiefly with a view to determining this very question: are Sun-spots elevations or depressions with respect to the photosphere? Howlett's conclusion is that his direct visual evidence decidedly refutes the depression theory. Indeed, in the rare cases where the perspective effect seemed to displace the (supposed) central umbra towards the center of the disk, the evidence was by no means necessarily in support of Wilson's view; for it was not proven that the umbra was central when the spot was further from the

[1] *A. N.*, **130**, 129–146, 1892. [2] *M. N.*, **55**, 73–76, 1894.

limb, and hence not subject to distortion by perspective. Moreover, Howlett found his visual observations to be corroborated by an examination he was permitted to make of the Greenwich solar photographs. He asserts that "instead of the penumbra of spots possessing *shelving sides*, sloping down towards the umbra, it (the penumbra) presents a *convex* surface; that is to say, a curve conformable to the general contour of the solar orb." Howlett also cites the names of Spoerer, Perry, Ranyard, and Whipple of Kew, as experienced observers who were satisfied of the incorrectness of the depression theory.

In the *Monthly Notices*[1] for April 1895, Sidgreaves brings the data obtained in the long series of visual observations at Stonyhurst to the support of Howlett's position. Of 187 single tests—of spots suitably situated for use for the purpose—he finds 140 opposed to Wilson's depression theory, and but 47 which might seem to confirm it, although he adds that at Wilson's own "valuation of their testimony, nearly all the 47 witnesses will go out of court." Individual spots are cited by Sidgreaves in which the visual and photographic evidence is convincing as to the elevated nature of the spots. Both Howlett and Sidgreaves refer to the impossibility that notches could be made in the edge of the disk by any ordinary spots, if they are depressions; while notches might occur as spots approached the limb, as was contended by Lalande, Kirchhoff and others, if spots are elevated masses of gas.

Per contra, however, it should be stated that the careful study of a large number of photographs, made some thirty years ago by De la Rue, Loewy, and Stewart, led them to adopt Wilson's view; and the measurements of Secchi and others, although difficult, were accepted as indicating a depression of the umbra of a few thousand miles below the photosphere.

In summing up this conflicting evidence from direct observation, I believe it must be admitted that the question of elevation of spots is at least an open one, and deserving careful consideration from students of solar physics.

[1] *M. N.*, **55**, 282–287, 1896.

We next examine the evidence afforded by the rotation of the Sun, as determined by the motions of the spots, faculæ, and photosphere itself, availing ourselves for this purpose of the valuable results recently published[1] by Stratonoff, in combination with the classical researches of Dunér[2] and Spoerer. Stratonoff had for an object the study of the motions of the faculæ, and he reaches the satisfying result that they exhibit an equatorial acceleration as do spots. I use the word satisfying, since the researches of Wilsing and of Bélopolsky were at variance on this point, as the measurements of the former failed to indicate an acceleration. It is incidentally that Stratonoff brings out the for us important fact that the order of the velocity of rotation is faculæ, spots, surface.

The motion of the surface was obtained with great precision by Dunér from measurement of the shifts (by Doppler's principle) of certain lines of the spectrum of the approaching and receding limbs of the Sun. Hence when we say "surface" we in fact refer to the level at which these absorption lines are produced. These lines were due to iron, hence we further limit our statement to the level of absorption of the stratum of iron vapor producing these lines ($\lambda 6301.72$, $\lambda 6302.72$), a stratum that may be reasonably supposed to lie very near the actual photospheric surface.

The results of Stratonoff, which have more recently been confirmed by Wolfer of Zurich in a memoir just received,[3] are more important for our question than at first might appear, since the faculæ have always been admitted to be at a higher level than the photosphere. Now we find that they share the peculiar law of rotation of the spots and surface (iron vapor level), and with numerical values next greater than those obtained for the spots, whose rotation was in turn faster than that of the "surface." Hence it is a logical inference that the

[1] *A. N.*, **140**, 113–119, 1896.

[2] *Sur la Rotation du Soleil*, Upsala, 1891.

[3] "Zur Bestimmung der Rotationszeit der Sonne." Separatabdruck aus der *Vierteljahrsschrift der Naturforschenden Gesellschaft in Zürich*, XLI., 1896.

level of the spots is intermediate between that of faculæ and surface.[1]

The evidence from thermal absorption that Sun-spots are situated above rather than below the level of the photosphere is perhaps the strongest that can be at present adduced. It may be stated quite simply. Measurements by Langley, by the writer, and by W. E. Wilson have furnished quite accurate tables[2] showing the decrease in the intensity of the thermal radiation at different distances from the center of the Sun's disk to the limb. The law of this absorption as found by experiment agrees quite satisfactorily with the theoretical law of Laplace after a suitable correction to make it accord with modern physical views.

The three observers mentioned have also measured the intensity of the thermal radiation from Sun-spots as compared with that from equal areas of adjacent photosphere. Now when a spot passes by the Sun's rotation from a point near the center of the disk toward the limb, it is found that the relative intensity of the spot increases. This might mean either that the radiation from the spot was greater, or that from the neighboring photosphere was less. But with our table showing the increase of absorption over the photosphere from the center of the disk outward, we can readily reduce our ratio to what it would be if the comparison area of photosphere were constantly at the center of the disk. On doing this we at once see that the thermal absorption over spots does not increase as we approach the limb in nearly the degree that is the case with the photosphere. The reasonable inference from this is that the spots are at a higher level than the photosphere, and hence less subject to the absorption of the Sun's atmosphere.

Still further, Langley and the writer have found instances in which the measured radiation from spots very near the limb *exceeded* that from the neighboring photosphere. As compared

[1] Since this paper was read, the August number of this JOURNAL has arrived, in which (p. 105) Wilczynski draws the same inference—"*that the spots are at a higher level than the photosphere*"—from Stratonoff's results.

[2] Collected on p. 302 of the new (1895) edition of Young's *The Sun*.

with an area of photosphere near the center of the disk, however, the radiation of the spot is always the less; as would be expected, unless we were to adopt the view, improbable in itself and contrary to these observations, that with the solar atmosphere removed Sun-spots would be actually more radiant than an equal photospheric area.[1] We are therefore again led to the conclusion that the level of spots is higher than that of the photosphere, so that less absorption occurs over the spots, making them in some cases appear to radiate more than neighboring areas of photosphere.

Thus far the spectroscope has not been called in witness in the case before us. A testimony from it contradictory to the view whose merits we are stating might well be considered fatal to that view; a favorable testimony would not be as convincing as an unfavorable one would be damaging.

Now it would appear that in fact the spectroscopic evidence is not competent to decide the question. The Wilsonian doctrine was accepted long before the spectrum of a Sun-spot had been examined; and when such examination was made, no contradiction was found. The spectrum of a spot shows firstly a general absorption, and secondly a selective absorption, not produced by smoke or solid particles, but, as has been pointed out by Young, Dunér, and others, by gases of less radiating power than the surface behind them. Large numbers of dark lines are broadened where the slit crosses a spot, and narrow down as they extend through the penumba; others are thinned over the umbra; sometimes those of calcium (H and K always), hydrogen, magnesium, and other metals are reversed; perhaps a majority of the lines are wholly unaffected.

To produce these effects it seems to be quite immaterial whether the absorbing masses of vapor are situated entirely above the photosphere or in cavities in it; in either case the white light that is absorbed is by all considered to be due to the

[1] It will be noted that in this discussion I have not spoken of relative temperature, a very different thing from relative thermal radiation, the quantity measured in such observations.

photosphere,—from an under stratum of it, according to the depression theory of spots; from its ordinary upper surface, according to the elevation theory. The same spectrum in umbra and penumbra would result from a "convex" mass of gases (say an anticyclone) above the condensed stratum, as from the gases poured into a cavity extending downward partly through that stratum (the photosphere). Arguments might be brought forward tending to show that the conditions in elevated masses of gas would be more favorable for the production of the observed effects, but in our present state of knowledge such reasoning would be based more on theory than on fact, and therefore I prefer to regard the evidence from the spectroscope as at present undecisive.

The query will probably be raised as to the bearing of the view presented in this paper upon the most prevalent Sun-spot theories; those of Secchi, Faye, Oppolzer. The first is summar, ized by Young as follows:[1] "Secchi believes them to be dense clouds of eruption-products settling down into the photosphere *near*, but not *at*, the point where they were ejected." Evidently the validity of this theory is not affected by the adoption of the view that the spots are gaseous masses above the photosphere, finally in their descent settling down upon it.

Faye's theory is based upon the relative drift of contiguous portions of the photosphere itself, due to the equatorial acceleration of the Sun's rotation. It would seem that the vortices might equally well take place in the gases above the photosphere as in that stratum itself. Certainly his view that the vorticular disturbance of the solar atmosphere propagates itself downwards conforms better to the elevation theory.

The Sun-spot theory of Egon von Oppolzer, based upon meteorological analogies, is in many respects a very promising one. To the writer no theory seems satisfactory which does not take into consideration these analogies in the terrestrial atmosphere. With the descent of masses of gas, under solar gravitation, their temperature must rise, at certain levels perhaps being

[1] *The Sun*, 1895, p. 182.

comparable with the photosphere in radiance, even if adiabatic conditions are but imperfectly realized, and this may possibly account for the reversed lines of calcium over spots. Oppolzer's may be called an anticyclone theory of Sun-spots, but its validity seems in no wise diminished if we consider that the descending currents do not penetrate the photosphere, but produce their absorption and run their course entirely above the photosphere.

The considerations that have been brought forward in this paper may seem to many to be insufficient for abandoning the doctrine of Wilson, but my purpose will be accomplished if observers shall give to this still unsettled question the attention that it fairly deserves.

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RÉSUMÉ OF SOLAR OBSERVATIONS MADE AT THE ASTROPHYSICAL OBSERVATORY OF CATANIA IN 1895.

By A. MASCARI.

DURING the year it was possible to utilize 331 days for observations of Sun-spots, 223 for faculæ and 224 for prominences. The methods of observation and the instruments employed have already been described in this JOURNAL.[1]

Table III. shows that the frequency of groups containing both pores and spots, and those containing only pores, diminished from the first to the third quarter. There was a slight increase in the last quarter, but the whole year shows a diminution when compared with 1894. This diminution was found also for the spots considered separately, and for the general frequency of the year, although the monthly results show certain irregularities, with the highest maximum of frequency for the entire year in October and the lowest minimum in November, when for three consecutive days we observed the Sun without spots, on one of these days even pores being absent.

The phenomenon of spots, considered by hemispheres, has shown a constant increase of spots and pores in the northern hemisphere, distributed in a smaller number of groups. The results are as follows:

	First Half		Second Half	
	Northern Hemisphere	Southern Hemisphere	Northern Hemisphere	Southern Hemisphere
Group of spots and pores..	2.75	2.95	2.38	2.60
Spots	4.91	3.77	4.34	3.49
Pores	16.6	14.4	17.1	9.9

The distribution in 10° zones of the spots and pores, for which the latitude has been calculated (Table II.) shows that

[1] *Ap. J.* **2**, 119.

TABLE I.

Day of month	January					February					March				
	Groups of spots and pores	Spots	Pores	Faculæ	Prominences	Groups of spots and pores	Spots	Pores	Faculæ	Prominences	Groups of spots and pores	Spots	Pores	Faculæ	Prominences
1	..	..	..	..	..	8	15	45	..	2	8	10	20	..	4
2	9	5	22	..	..	8	11	63	10	4	..	..	..	..	..
3	8	4	23	..	1	8	15	19	..	..	6	7	33	..	..
4	..	..	..	..	..	7	10	55	10	2	7	9	26	7	3
5	6	11	34	..	..	7	9	41	..	2	7	10	9	9	3
6	5	7	58	..	4	4	5	31	7	..	5	8	18	12	2
7	7	7	40	5	4	4	8	30	13	..	4	8	25	..	3
8	4	6	46	10	1	4	9	13	..	4	4	8	24	..	2
9	6	6	44	8	1	6	5	28	11	3	4	8	35	9	4
10	6	8	39	8	0	6	4	22	..	3	6	8	49	14	2
11	7	8	59	10	..	4	5	31	11	9	..	..	..	..	..
12	4	7	33	11	..	..	..	..	..	..	..	..	..	..	..
13	3	4	31	9	1	4	6	16	..	..	5	10	39	..	..
14	3	2	18	6	3	6	5	11	10	3	4	11	26	6	4
15	5	2	15	11	4	..	..	..	..	..	5	8	7	..	..
16	4	1	20	..	..	5	6	29	13	4	..	..	..	..	..
17	3	2	25	11	1	6	6	18	..	..	..	..	..	..	..
18	5	6	21	11	2	7	9	11	..	2	5	9	22	10	4
19	4?	10?	6?	..	..	6	9	10	..	..	4	11	19	12	5
20	4	7	41	14	3	6	5	42	..	2	3	10	29	..	5
21	4	8	56	12	2	..	..	..	..	..	4	12	27	..	3
22	4	6	39	..	..	6	9	28	13	0	4	12	24	..	2
23	6	6	77	..	..	..	..	..	..	..	5	8	19	8	3
24	7	6	52	16	2	10	13	53	..	..	6	7	25	12	5
25	7	7	83	14	1	..	..	..	..	..	5	10	13	..	5
26	8	12	72	13	4	10	12	34	..	..	4	9	13	10	..
27	9	15	52	..	..	10	13	35	..	..	6	8	6?	..	3
28	8	18	83	..	..	10	13	32	8	3	5	8	14	..	..
29	8	19	52	..	..	..	..	..	..	..	4	7	29	12	4
30	10	20	52	..	..	..	..	..	..	..	4	8	31	..	4
31	..	..	..	..	..	..	..	..	..	..	4	9	14	..	..

the most active region in both hemispheres was the zone $\pm 10° \pm 20°$, and that no spots were seen in the zones higher than $\pm 40°$. The extreme latitudes reached by the spots this year were $+32°.0$ and $-31°.3$

The mean latitude of the spots for the different quarters is diminishing, which illustrates the tendency of the phenomenon to seek lower latitudes; calculation gives $14°.5$, $14°.3$, $13°.6$ and $12°.6$ for the mean latitude of the spots in successive quarters.

TABLE I.—*Continued.*

Day of month	April					May					June				
	Groups of spots and pores	Spots	Pores	Faculæ	Prominences	Groups of spots and pores	Spots	Pores	Faculæ	Prominences	Groups of spots and pores	Spots	Pores	Faculæ	Prominences
1	3	7?	2?	..	..	6	9	22	..	..	5	7	25	17	1
2	7	9	21	9	4	8	10	27	10	3	6	11	24	15	..
3	6	9	25	7	7	4	5	19	11	..	6	6	35	21	4
4	5	9	19	..	..	3	5	10	12	..	5	5	41	14	3
5	8	7	24	9	9	2	3	21	16	..	6	8	67	16	2
6	6	4	37	11	3	..	..	..	..	..	6	11	58	..	..
7	..	..	..	..	..	..	..	..	..	..	6	8	62	16	3
8	8	3	40	11	3	4	3	13	12	3	6	12	31	17	3
9	7	6	34	9	2	3	2	12	..	..	6	8	51	17	2
10	8	10	27	7	3	4	3	19	13	2	6	10	24	14	4
11	9	12	20	12	5	3	2	7	11	1	5	7	18	11	3
12	8	10	22	8	3	7	6	10	12	2	4	10	10	..	1
13	7	9	33	11	0	5	3	18	14	7	5	9	25	16	1
14	5	5	27	16	..	5	6	27	15	4	5	11	33	18	2
15	5	3	24	11	6	4	4	30	11	4	5	21	44	15	6
16	5	7	19	..	..	3	6	12	..	..	5	16	69	14	3
17	5	9	34	..	2	5	7	20	13	4	4	15	52	14	3
18	3	7	18	12	5	4	7	12	..	4	4	15	34	14	2
19	..	..	..	..	..	5	5	24	10	2	4	9	31	15	2
20	4	8	33	9	1	5	6	33	11	3	4	9	11	15	6
21	5	13	18	..	3	5	8	27	..	4	4	9	38	17	2
22	6	15	27	..	3	5	8	47	13	5	5	10	27	16	3
23	8	12	46	11	0	7	12	44	..	..	5	11	28	11	2
24	6	9	55	..	..	7	18	24	..	..	6	7	45	14	4
25	6	10	36	8	2	..	..	..	..	..	7	7	31	13	2
26	8	14	49	10	2	9	14	39	..	..	5	4	29	..	1
27	7	14	43	..	..	10	13	55	13	5	5	5	21	16	1
28	..	..	..	..	..	9	16	45	13	2	5	8	26	14	3
29	7	18	36	..	..	7	13	28	16	6	7	10	27	13	2
30	6	14	32	..	..	7	10	20	12	3	6	8	34	12	1
31		..	..	..	..	8	8	25	14	1	..	..	..	..	..

In the case of the faculæ we see from Table II. that there were present in each solar hemisphere two maxima, almost symmetrically arranged on each side of the equator in the zones $\pm 10° \pm 20°$ and $\pm 70° \pm 80°$, with a secondary mininum in the same zones $\pm 50° \pm 60°$, in addition to the two other minima in the zones of the equator and the poles. In comparing these results with those of last year it is seen that the maximum between $\pm 10° \pm 20°$ remained stationary, as did also the other secondary maximum of the northern hemisphere

TABLE I.—*Continued.*

Day of month	July					August					September				
	Groups of spots and pores	Spots	Pores	Faculæ	Prominences	Groups of spots and pores	Spots	Pores	Faculæ	Prominences	Groups of spots and pores	Spots	Pores	Faculæ	Prominences
1	7	6	36	14	6	5	8	34	9	3	4	4	19	9	4
2	8	7	12	16	7	6	10	36	10	5	5	6	27	10	6
3	5	5	10	..	..	6	14	58	6	7	5	5	15	9	8
4	5	4	20	11	4	7	15	51	..	4	7	4	26	10	8
5	5	4	6	..	6	6	17	59	..	..	6	5	17	8	6
6	5	2	21	10	5	7	20	115	8	3	6	11	22	9	8
7	6	7	12	9	2	6?	17?	31?	..	4	5	9	15	12	5
8	7	9	39	11	1	7	20	60	9	3	5	7	19	..	5
9	9	11	54	..	1	6	12	111	10	4	5	5	34	9	3
10	7	10	33	14	3	7	13	84	8	5	5	4	7	13	4
11	7	13	45	13	0	6	15	78	10	3	3	3	4	10	1
12	4	10	19	17	2	6	12	74	9	3	3	2	6	10	5
13	6	14	38	..	1	6	10	38	11	3	5	3	27	10	3
14	7	11	36	15	5	6	8	23	10	1	5	7	33	13	3
15	6	9	28	8	6	4	4	15	8	4	6	5	46	11	2
16	5	6	10	15	4	5	7	17	8	9	6	4	36	..	5
17	5	4	22	10	10	5	7	35	12	2	3	3	15	8	4
18	5	4	9	11	4	3	8	16	9	8	3	3	7	11	..
19	2	1	7	6	6	3	10	19	..	6	2	3	2	..	..
20	3	1	6	9	4	4	7	31	10	4	4	3	15	10	7
21	2	4	9	9	7	2	5	34	10	7	3	2	21	10	8
22	2	4	3	12	0	2	5	38	11	9	5	2	25	9	3
23	4	3	5	12	5	3	4	21	7	6	3	6	5	..	4
24	..	..	..	..	..	5	9	21	8	6	5	6	9	8	4
25	1	1	0	8	6	6	9	18	11	3	7	5	13	11	2
26	1	1	1	10	6	4	6	6	12	1	8	8	14	8	4
27	4	2	14	6	3	..	..	..	..	..	9	11	30	8	4
28	3	2	23	..	3	..	..	..	..	..	7	10	37	7	2
29	3	4	15	11	5	3	4	16	10	4	6	12	40	..	4
30	2	3	27	8	5	3	5	8	8	4	7	17	43	12	2
31	3	6	35	8	..	4	4	7	..	3	..	..	..	..	..

in the zone + 70° + 80°. The other secondary maximum of the southern hemisphere, which in 1894 was in the zone − 60° − 70°, has moved to a higher latitude, in the zone − 70° − 80°. The minima which in 1894 were in the northern zone + 60° + 70°, and in the southern zone − 40° − 50°, were in 1895 in + 50° + 60° and − 50° − 60° respectively. The mean frequency of the faculæ was greatest in the northern hemisphere during the first half of the year, and during the second half about equal in each hemisphere. Thus for the entire year the

TABLE I.—*Continued.*

Day of month	October					November					December				
	Groups of spots and pores	Spots	Pores	Faculæ	Prominences	Groups of spots and pores	Spots	Pores	Faculæ	Prominences	Groups of spots and pores	Spots	Pores	Faculæ	Prominences
1	8	17	85	8	3	6	13	66	..	..	3	1	6	8	..
2	7	29	20	..	4	..	..	..	..	..	..	..	..	..	..
3	7	27	70	11	5	6	15	67	..	..	4	3	19	17	6
4	6	24	28	..	1	..	..	..	..	..	3	5	15	12	7
5	5	17	41	..	4	..	..	..	..	..	5	5	17	8	1
6	6	12	66	8	1	3	9	22	10	3	6	8	20	11	3
7	3	12	46	9	2	3	6	37	9	3	6	8	10	..	3
8	3	11	35	5	2	2	2	13	7	2?	7	12	31	..	..
9	3	5	36	14	4	1	1	3	11	3	7	14	24	..	2
10	2	3	2	10	..	0	0	0	12	..	8	10	42	..	3
11	2	3	2	..	1	1	0	4	11	2	9	12	62	11	2
12	6	3	10	13	2	1	0	3	8	4	12	11	81	12	..
13	3?	3?	4?	..	3	2	2	9	13	3	11	12	71	..	..
14	4	3	5	14	5	5	5	18	8	3	9	8	55	..	6
15	4	2	11	10	1	5	7	17	12	5	6	8	25	..	6
16	2	2	8	13	5	..	..	..	..	..	8	10	33	..	..
17	2	2	6	6	4	6	5	24	10	4	8	7	33	..	..
18	..	..	..	..	..	5	3	27	8	1	..	..	..	..	..
19	2	1	6	..	..	6	5	18	8	5	7	7	21	10	..
20	..	..	..	..	..	7	5	13	10	5	7	8	12	12	..
21	4	7	9	..	2	4	4	8	8	3	5	9	45	..	1
22	4	12	2	..	3	..	..	..	..	..	8	14	28	..	5
23	5	15	26	..	..	5	5	1	..	..	7	14	44	..	..
24	5	15	50	12	3	..	..	..	..	..	9	13	43	9	1
25	6	19	21	..	5	6	6	8	13	2	8	11	20	..	..
26	4	16	58	11	..	5	6	0?	..	..	9	10	35	..	..
27	4	16	107	9	5	..	..	..	..	..	7	9	33	9	..
28	4	14	86	..	2	4?	5?	2?	..	7	..	..	..	..	..
29	6	17	70	11	0	4	2	4	13	..	4	6	19	..	1
30	6	15	71	12	2	..	..	..	..	..	3	8	16	14	2
31	7	16	55	..	1?	..	..	..	..	..	4	8	19	13	..

northern hemisphere was the richer in faculæ, the reverse of what was observed in 1894, when the southern hemisphere had the preponderance.

The great differences between our own observations and those of others regarding the number and distribution of the faculæ is due in part to the method of observation and the size of the projected image. We always see the faculæ in all latitudes, but in the higher latitudes the faculæ near the limb are never seen in long trains : they are usually isolated or in small groups.

TABLE II.

Heliographic Latitude		Spots					Faculæ					Prominences				
		First Quarter	Second Quarter	Third Quarter	Fourth Quarter	Year 1895	First Quarter	Second Quarter	Third Quarter	Fourth Quarter	Year 1895	First Quarter	Second Quarter	Third Quarter	Fourth Quarter	Year 1895
North	80° to 90°	0	0	0	0	0	8	37	21	20	86	0	0	0	1	1
	70 " 80	0	0	0	0	0	16	38	31	27	112	0	0	0	0	0
	60 " 70	0	0	0	0	0	17	38	30	20	105	0	0	0	0	0
	50 " 60	0	0	0	0	0	14	34	34	20	102	1	6	21	4	32
	40 " 50	0	0	0	0	0	20	41	30	25	116	5	9	70	24	108
	30 " 40	0	1	0	0	1	23	57	43	22	145	13	32	43	20	108
	20 " 30	3	3	5	5	12	26	66	42	49	183	17	30	41	18	106
	10 " 20	23	24	16	20	76	41	81	93	49	264	21	23	22	15	81
	0 " 10	10	9	9	7	38	21	52	33	30	136	15	25	22	11	73
South	0° to 10°	9	7	11	6	31	33	65	46	28	172	13	19	24	17	73
	10 " 20	19	17	20	22	73	44	77	82	50	253	16	13	26	12	67
	20 " 30	10	7	5	2	24	33	55	68	43	199	12	25	29	17	83
	30 " 40	1	1	0	0	2	22	44	44	22	132	18	9	33	19	79
	40 " 50	0	0	0	0	0	16	23	32	13	84	10	8	27	16	61
	50 " 60	0	0	0	0	0	8	25	17	15	65	6	1	5	0	12
	60 " 70	0	0	0	0	0	24	24	29	11	88	0	0	0	0	0
	70 " 80	0	0	0	0	0	20	43	38	28	129	0	0	0	0	0
	80 " 90	0	0	0	0	0	10	28	29	21	88	0	0	1	0	1

During the year the Sun was observed here on six days without prominences higher than 30″. Table III. shows that during the first quarter the frequency of the prominences was nearly equal in the two hemispheres, though during the remainder of the year it was greatest in the northern hemisphere. Taken as a whole there was a slight and increasing activity of the prominences in the first three quarters, although in comparing these results with those of 1894 it is evident that the frequency of the prominences continued to diminish in 1895. Their distribution in 10° zones of latitude shows the almost complete absence of the phenomenon in high latitudes, the minimum in the equatorial zones and the most marked activity in the northern zone $+30° + 50°$ and in the southern zone $-20° - 30°$.

The following conclusions may be drawn from our observations in 1895 :

TABLE III.

1895	Mean Frequency								
				Of prominences			Of faculæ		
	Of groups of spots and pores	Of spots	Of pores	In northern hemisphere	In southern hemisphere	In both hemispheres	In northern hemisphere	In southern hemisphere	In both hemispheres
January	5.83	7.86	42.6	0.75	1.37	2.12	5.12	5.44	10.56
February	6.57	8.74	30.3	1.71	1.36	3.07	5.00	5.60	10.60
March	4.92	9.00	22.9	1.80	1.70	3.50	4.50	5.58	10.08
April	6.22	9.37	29.7	2.26	1.05	3.32	4.82	5.24	10.06
May	5.50	7.57	24.6	1.84	1.58	3.42	6.80	5.80	12.60
June	5.23	9.43	34.8	1.68	0.89	2.57	8.37	6.63	15.00
July	4.63	5.60	19.8	2.64	1.54	4.18	5.36	5.56	10.92
August	4.90	9.69	35.3	2.82	1.57	4.39	4.46	4.88	9.33
September	4.93	5.83	20.5	2.36	2.07	4.43	4.64	5.16	9.80
October	4.48	11.66	35.7	1.56	1.24	2.80	5.59	4.76	10.35
November	3.95	4.82	16.5	1.63	1.81	3.44	5.12	4.94	10.06
December	6.48	8.59	30.0	1.87	1.40	3.27	6.15	5.08	11.23
First Quarter	5.74	8.51	32.3	1.44	1.50	2.94	4.89	5.53	10.42
Second Quarter	5.64	8.80	29.8	1.89	1.14	3.03	6.94	6.00	12.94
Third Quarter	4.82	7.01	25.1	2.61	1.73	4.33	4.82	5.20	10.03
Fourth Quarter	5.06	8.66	28.3	1.66	1.45	3.11	5.57	4.91	10.49
Year	5.30	8.22	28.8	1.99	1.47	3.46	5.60	5.43	11.03

(1) The Sun has exhibited a further diminution in the phenomena of spots and prominences as compared with the year 1894.

(2) The maximum activity of the spots and the primary maximum of the faculæ were in the same zone of latitude $\pm 10°$ $\pm 20°$.

(3) The double maximum in each hemisphere of the faculæ, observed in 1894, has been repeated this year.

(4) The activity of all the phenomena, spots, faculæ and prominences, was greater in the northern than in the southern hemisphere.

(5) The discordance noted in 1894 between the distribution in latitude of faculæ and prominences was observed again in 1895.

RESEARCHES ON THE ARC SPECTRA OF THE METALS. II. THE SPECTRUM OF TITANIUM. II.[1]

By B. HASSELBERG.

7. After this discussion I give in the following table the results of the determinations of wave-lengths in the titanium spectrum. As in the case of the similar table for chromium, the successive columns contain the wave-lengths of the titanium lines; the standard lines in the solar spectrum which were used as reference lines, taken from Rowland's catalogue;[2] the estimated intensities of the titanium and solar lines, on a scale of 1:6; and the remarks which were made in the course of the investigation. The last column contains the measurements of Thalén, reduced to Rowland's scale.[3]

λ	R.-Å.	λ	R.-Å.	λ	R.-Å.
390–400	0.85	460–470	0.88	530–540	1.25
400–410	0.73	470–480	0.84	540–550	1.30
410–420	0.56	480–490	0.90	550–560	1.43
420–430	0.63	490–500	0.80	560–570	1.28
430–440	0.80	500–510	0.96	570–580	1.17
440–450	0.85	510–520	1.11	580–590	1.22
450–460	0.95	520–530	1.09	590–600	1.32

[1] Continued from page 134. With three Plates.

[2] *A. and A.*, 1893.

[3] The reduction factors are given in the following table, obtained by comparing a large number of iron lines (about 100) according to the measures of Kayser and Runge, and of Thalén. Each difference is the mean of from four to six individual differences within each division of 100 tenth-meters of the spectrum. The smaller variations of the figures are of little importance here, where it is only a question of the nearest tenth of a tenth-meter.

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
3477.33		2.3	2		
	3478.00				
79.07		1.2	—		
80.67		2+	1		
81.83		1	—		
85.84		1.2	1		
89.90		1.2	2.3	⊙ line probably a close double. { Companion toward R=Ti " " V=Fe } K.−R. 89.81	
91.20		3	3	very sharp.	
	3491.47				
93.44		1	1		
95.88		2	2.3		
99.24		2+	1		
3500.48		1.2	2		
05.02		3	2		
06.76		2	2	⊙ line close double K.-R.: 06.66 } companion toward R, Ti. " " V, Fe.	
07.55		1.2	1.2		
10.98		3	2	very sharp.	
11.74		1	1		
12.23		2	1	very diffuse and broad, Ti?	
16.97		1.2	1		
	3518.48				
20 15		1.2	2.3	diffuse.	
20.39		2	2	sharp.	
24.37		2	2		
25.28		1.2	1		
26.18		1.2	2.3	also a weak Fe line.	
27.62		1.2	—		
30.53		3	2	very diffuse; in Fe also an insignificant line.	
35.56		2.3	2		
	3540.27				
42.69		1.2	1.2		
45.11		1	1		
47.15		2.3	1		
56.32		1.2	?	diffuse.	
58.66		2	3	also a strong Fe line.	
61.72		1.2	1.2		
	3564.68				
66.16		1.2	1.2		
73.85		2	2.3	⊙ line double { 73.85 Ti 74.05 Fe } K.-R.: 74.06.	
74.38		2	1.2		
76.00		1	1		
78.40		1.2	?		
80.40		1.2	1		
	3583.48				
94.13		1	1		
96.17		2.3	2		

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
3598.87		2.3	1.2		
99.25		2.3	1.2		
3601.31		1	1		
01.52		1	1		
03.98		1.2	2	⊙ line close triple.	
04.39		1.2	1.2	very sharp, ⊙ line double { 04.40 Ti .50	
05.46		2	2.3	very sharp.	
06.18		1—	?	probably a weak ⊙ line.	
07.26		2	1	diffuse.	
09.72		1+	—		
10.29		3	3	Ti appears to be a little to one side of the ⊙ line.	
——	3612.21				
12.40		1.2	—	diffuse.	
13.89		2	?	at the edge of the ⊙ line 13.95.	
14.35		2	—	diffuse.	
20.15		1	1		
21.37		2	2	diffuse; ⊙ line double { 21.20 .37	
23.25		2	—	diffuse; close to 23.40 (Fe).	
24.97		2	2	very sharp; ⊙ line double { 24.87 Ni 25.00 Ti, Fe K.-R.: 25.01.	
26.22		1.2	1	very sharp; ⊙ line double { 26.22 .30	
33.60		2	1		
35.33		2	2		
35.61		4.5	2	very diffuse.	
36.05		1	1		
38.10		2	—		
——	3640.53				
41.48		2.3	2	very sharp.	
42.82		5	2.3	very diffuse; ⊙ line double { 42.82 Ti .92	
44.87		2	2	⊙ line close double { 44.87 Ti .95	
46.32		2.3	1.2	very sharp.	
53.61		5	2	very diffuse.	
54.72		3	2	⊙ line close double { 54.72 .80	
58.22		3.4	2.3	⊙ line close double { 58.20 .25	
——	3658.68				
59.91		2.3	2.3	very sharp.	
60.75		3	1.2		
62.37		2.3	2.3		
63.82		1	1	⊙ line distinct; not in R.	
66.71		1	1.2		
69.08		2.3	2	⊙ line close double. Separation impossible.	
71.82		3	1.2		

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
3677.90		1	—		
79.88		2	1	diffuse.	
81.38		1.2	1.2		
——	3684.25				
85.30		4	3	diffuse.	
86.10		2	—	not coincident with 86.15 (Fe). K.-R.: 86.16.	
87.48		2	?	at edge of strong ⊙ line 87.63, K.-R.: 87.64	
88.19		1	1		
90.04		3	1.2		
92.35		2	—	diffuse; Ti?	
94.58		2.3	1		
——	3695.19				
97.05		1	1		
98.33		1.2	—		
3700.22		1.2	—		
02.42		2	2	coincidence not certain.	
03.13		1	—		
04.42		2	1.2	⊙ line double { 04.42 .50	
04.84		1	1.2		
06.37		1.2	2		
07.68		1.2	2	⊙ line double. With which companion?	
08.83		1.2	—		
10.10		2	1.2		
——	3716.57				
17.53		1.2	2		
21.75		2	2	⊙ line belongs to a group of 5 close lines	
22.70		2.3	4	also in Fe a line of medium strength.	
24.70		2.3	2		
25.28		2.3	2		
29.92		3.4	2.3		
33.96		1+	1		
35.84		1.2	1		
39.17		1.2	—		
41.19		3	2		
41.78		2	2	sharp; ⊙ has { 41.65 .78	
——	3747.09				
48.26		2	?		
53.00		3.4	2		
53.75		2.3	2.3	also an Fe line of medium strength.	
57.82		2	2	very sharp.	
59.42		3.4	3		
61.46		3.4	3	⊙ line probably double; coincidence with the component toward violet.	
62.01		1	2		
66.60		1	1		
71.80		2.3	1.2		
——	3770.12				
76.20		1	1.2		

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
3782.26		1.2	1.2		
86.20		2.3	1	⊙ has a triple { 86.12 Fe, .20 Ti, .34 Fe } K.-R.: { 86.14, .37 }	
86.44		1.2	1.2		
89.46		2	?		
——	3794.02				
96.06		2	1		
98.47		1.2	1	sharp.	
3801.25		1.2	1	very sharp.	
01.73		1	1		
05.25		1	—		
05.64		1	1		
06.19		1	—		
06.60		1	1		
07.37		1	—		
07.93		1	—		
11.56		1	1		
13.42		1.2	1	{ ⊙ triple { 13.42, .54, .65 }	
13.54		1.2	2	}	
14.72		2	2	⊙ double { 14.60, .72 } Fe, Ti K.-R.: 14.73	
15.01		1.2	1	⊙ line companion to 14.90.	
17.78		2	2	⊙ line probably double { 17.78, .90 }	
18.38		2	1.2	⊙ line double { 18.40, .50 }	
——	3821.32				
21.86		1	1.2	sharp { ⊙ has a triple { 21.86 Ti, 22.06 Fe, 22.16 Ti }	
22.16		2.3	1.2	}	
23.03		1	1.2		
27.12		1.2	1		
27.61		1	?		
27.80		1	?		
28.16		1.2	1	in the shadow of the strong ⊙ line 27.95 (Fe).	
28.31		2	2		
29.87		1.2	?	falls on a strong band of ⊙.	
33.33		2	1	sharp.	
33.80		2	1	sharp.	
34.06		1.2	?	does not appear to coincide with ⊙ line 34.10.	
36.22		1.2	2	sharp.	
36.90		2	1.2		
40.48		1	—		
40.90		1	1		
41.79		1	—		

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
3842.77		1	—	does not coincide with the ⊙ line 42.82.	
——	3843.40				
45.28		1.2	1.2		
46.57		2	2	also in Fe.	
48.48		1.2	1.2	very sharp.	
53.18		2.3	1	diffuse.	
53.87		2.3	1	diffuse.	
55.99		1.2	2		
58.04		1.2	1.2		
58.26		2.3	1.2	diffuse.	
60.61		1.2	1		
61.25		2	?	between the ⊙ lines { 61.20 .30	
61.89		1.2	1.2	diffuse.	
62.98		2.3	1	diffuse.	
64.66		1	1.2		
66.17		1	2		
66.60		3	1.2	diffuse.	
67.92		2	1	diffuse; coincidence not absolutely certain.	
68.56		2.3	1.2	diffuse.	
69.13		1	—		
69.47		2.3	—	diffuse.	
69.75		1.2	2.3	⊙ line distinct, sharp, probably double, also a line in Fe.	
70.28		1.2	2		
73.40		2.3	—	diffuse; at edge of ⊙ line 73.25 (Co, Ni).	
74.32		2	?	falls in the shading of 74.10.	
——	3875.23				
75.44		3	2	diffuse; ⊙ line double. { 75.45 .55 } coincidence not quite certain.	
77.75		1.2	—	diffuse.	
81.85		1.2	2.3	⊙ line appears triple, the Ti line lies on the red edge.	
82.28		3	2.3	diffuse.	
82.49		2.3	2.3	diffuse.	
83.02		3.4	2	diffuse.	
88.20		2	1	diffuse; between two bismuth lines { 88.05 .34	
90.12		2	1.2	sharp; ⊙ line double { 90.06 .12	
95.42		3.4	1	coincidence uncertain.	
——	3897.60				
98.68		2	1	very sharp; ⊙ has { 98.58 .68	
3900.68		2.3	3	also an unimportant Fe line.	
01.13		2.3	1.2		
04.95		3.4	2.3		
11.34		2	1	diffuse.	
13.58		2.3	2 3		
14.45		2.3	2		
14.86		1.2	1	sharp.	

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
3916.00		1.2	?		
16.27		1.2	—		
19.95		1.2	1		
21.56		2	1.2		
——	3924.67				
24.67		2.3	2		
26.48		2.3	1		
30.02		2.3	1.2		
34.37		1.2	?		
38.18		1	1		
——	3942.55				
47.90		3	1.2	⊙ line close double { 47.80 .90	
48.80		3.4	1.2	⊙ has a triple { 48.80 Ti .90 Fe 49.00	
56.45		3.4	2	⊙ has a distinct group { 56.45 Ti .55 .75 } Fe 57.10	
58.33		3.4	2		
62.98		2.3	1.2	very sharp.	
64.40		2.3	2	very sharp.	
——	3971.48				
81.91		3.4	2	also in Fe an insignificant line.	
82.62		2.3	1.2	⊙ line close double { 82.62 .70	
84.48		1.2	—		
85.57		1.2	—	diffuse.	
85.76		1.2	—	diffuse.	
89.92		4	2	⊙ line close double { 89.92 Ti 90.00 Fe K.-R.: 90.01	
94.84		1.2	—	diffuse.	
98.77		4	2.3		
99.53		1.2	—	diffuse.	
4002.63		2	—	diffuse.	
——	4003.91				
03.99		2	—	diffuse.	
06.14		1.2	—	diffuse.	
07.38		1.2	—	diffuse.	
08.20		2	—	diffuse.	
09.06		3	2		
09.80		2	—	between the ⊙ lines 09.70 and 09.87 (Fe).	
12.55		1.2	2	sharp.	
13.72		2.3	—	diffuse.	
15.56		2	—	diffuse.	
16.44		1.2	—	diffuse.	
17.13		1	—		
17.93		2	—		
21.98		2.3	—	diffuse.	

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
4024.71		3	2		
25.26		1	2		
26.64		2.3	—	diffuse.	
27.66		1.2	—		
28.48		1.2	2		
——	4029.79				
30.60		2.3	—	diffuse.	
34.05		1.2	1	⊙ line exceedingly weak; not in R.	
35.05		1.2	1	group of exceedingly weak ⊙ lines not in R.	
35.98		2	—		
——	4048.88				
53.96		1.2	1.2		
55.18		2.3	2	also a weak Fe line.	
57.76		1.2	?	diffuse; in the shadow of 57.60.	
58.28		2	?	diffuse; ⊙ has 58.37 (Fe).	
60.42		2.3	1.2		
——	4062.60				
64 36		2	1	very sharp.	
65.23		2	2	very sharp.	
74.50		1	—		
76.50		1	—		
77.30		1	1		
78.61		3	1.2	⊙ line double { 78.50 Fe .61 Ti	
79.85		2	?		
82.57		2.3	2	very sharp.	
——	4088.71				
90.73		1	1.2		
92.83		1.2	2		
95.65		1	1		
99.32		2	1		
99.94		1.2	1.2	sharp.	
4101.08		1	1		
05.31		1.2	1.2		
09.92		1.2	2.3	also Fe.	
11.91		2.3	2	sharp.	
12.86		2.3	1.2	very sharp.	
15.32		2	2	sharp.	
16.64		1.2	1.2		
21.79		1.2	—	sharp.	
——	4121.96				
22.31		2	1		
23.42		2	1.2	diffuse.	
23.68		2.3	1.2	diffuse.	
27.67		2.3	?	⊙ has 27.75=Fe. This line has perhaps a weak companion 27.69.	
28.20		2	?		
29.30		1.2	1.2		
31.38		1.2	?		
34.60		1.2	2	⊙ line double { 34.50 .60	

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
4137.39		2.3	1	diffuse.	
43.16		1.2	?	diffuse; a group of fine ⊙ lines lies here; coincidence impossible to decide upon.	
51.11		2.3	1		
——	4157.94				
59.79		2.3	1		
61.67		1	2		
63.80		2.3	2		63.6
64.27		1.2	1		
64.80		1	1		
66.45		2	1		
69.46		2	1		
71.15		2.3	1	⊙ has { 71.00 Fe .15 Ti	71.6
72.04		2	2		
73.66		1.2	2	⊙ line close double 73.55. .66 Ti.	
74.20		1	1		
74.61		1.2	1		
83.45		1.2	1		
——	4185.05				
86.27		3.4	1.2		85.6
88.84		2	?		
4200.88		1.2	1	⊙ has a broad band of fine lines; decision impossible.	
03.58		2	1	⊙ line exceedingly weak.	
11.85		1.2	1		
——	4215.65				
24.96		2	?		
27.80		2	?		
38.00		2.3	1		37.1
45.66		1.2	—		
49.29		2	?		
51.77		2	1		
51.93		2	1		
——	4254.49				
56.18		2.3	1		
58.68		2	1	⊙ line triple { 58.68 .80 .95	
60.91		1	2		
61.75		2 +	?	between the ⊙ lines 61.65 and 61.90.	
63.28		3	1.2		63.6
65.42		1.2	2		
65.85		1.2	1		
66.37		1.2	1		
——	4267.94				
70.30		2	1.2		
72.57		2	?		
73.45		1.2	2		
74.73		3	1.2		73.6

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
4276.55		2	1	⊙ has a weak triple { 76.15 .35 .55	
78.34		2	2.3		
78.95		1	—		
80.17		1.2	1.2		
81.49		2.3	1		
82.85		3	1.2	⊙ line close double; coincidence with the component toward V.	82.6
85.15		2.3	2		
86.15		3.4	2		
87.55		3.4	2		87.6
88.29		1.2	1.2		
89.23		3.4	2		
90.07		1.2	1.2		
90.37		2.3	2.3		
91.07		3	3	"large" (Th.)	91.3
91.32		2	2		
—	4293.24				
94.28		3	3.4	⊙ line perhaps double. K.-R. give a strong Fe line at 94.33.	94.4
95.91		3.4	1.2		95.6
98.82		3.4	1.2		
99.38		3	2		
99.79		3	1.2	Thalén observed these seven lines as a broad band.	99.8
4300.19		3	2		
00.73		3.4	2		
01.23		3.4	3		
02.08		2.3	2		
06.07		4	2.3		06.0
08.64		1.2	?		08.3
11.80		1.2	?		
13.01		3	2.3		13 3
14.50		2.3	1.2	⊙ line very close double { 14.40 .50	14.3
14.95		3.4	2		
15.15		2	2	⊙ line close double { 15.15 Ti .28 Fe	
16.96		2	1.2		
—	4318.23				
18.83		3.4	3	Also a strong Ca line K.-R.: 18.83.	18.8
21.12		1.2	2		20.8
21.82		3	1.2		
25.30		3	1.2	Thalén "large."	24.3
26.50		3	1.2		
27.12		1.2	1		
30.85		1.2	2		
34.98		1.2	1	⊙ line the middle line of a weak triplet.	
38.05		3	2.3		38.3
38.62		1	1		
41.51		1.2	2		

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
——	4343.98				
4343.93		1	—		
44.47		1.2	2		
46.26		2	1		
46.76		1	2		
50.99		1	1		
53.01		1	—		
54.20		1.2	?		
55.44		1	1		
60.60		2	2		
61.31		1	—		
67.81		1.2	2	⊙ line strong double { 67.70 .80	
69.11		1.2	?		
69.82		2.3	?		
72.54		2	1		
74.97		1	1.2		
75.61		1	?		
——	4376.10				
79.40		2	3		
84.85		2	2	⊙ line close double { 84.85 .95	
87.00		1	1.2		
88.22		1.2	?		
88.69		1	?		
90.11		2	2		
94.04		3	1	⊙ line belongs to a weak triple.	93.8
94.19		1.2	—		
95.17		3.4	3		
95.99		1.2	2		
99.92		2.3	2.3		99.3
4400.74		1.2	1.2		
04.42		3	1.2		03.8
04.57		2	1		
05.07		2	?	In the shadow of 04.95 between this line and its companion 05.15. Line 04.95 = Fe scarcely visible on the plate.	
05.86		2	1		
07.85		1.2	2.3	⊙ line close double { 07.85 .95	
——	4407.85				
08.39		1.2	2		
08.70		1.2	2.3	⊙ line close double { 08.60 .70	
09.41		1	1.2		
09.71		1	1.2		
12.61		1.2	1	line 11.8 (Th.) missing.	
14.29		1	1		
16.70		2	1.2		
17.46		3	1.2	{	18.6
17.88		2.3	2.3	{	

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
4418.52		1.2	2		
21.92		2	1		
23.00		2.3	1		
24.58		1.2	—		
——	4425.60				
26.01		1.2	1	⊙ line narrow double { 25.90 / 26.00	
26.24		2.3	1.2		
27.28		4	2.3		27.6
30.19		1.2	1		
30.55		2.3	1		
31.46		2	—		
32.76		1.2	2		
33.75		1.2	1		
34.15		3	1.2		
34.54		1	1		
36.75		2	1		
38.38		1.2	?		
40.49		2.3	1		
41.45		2	1		
41.86		1.2	2		
43.16		1.2	1		
43.97		3	3		43.8
44.41		1.2	1.2		
44.72		1	2		
——	4447.90				
49.32		3	2	{	50.3
50.66		2	2	{	
51.07		3	2		
51.40		1.2	1	⊙ line wanting in R.	
53.48		3	2		53.3
53.87		2.3	1.2		
55.48		3	2		55.8
57.59		3.4	2	⊙ line double { 57.60 / .70	58.3
59.62		1	—		
62.26		1.2	—		
63.52		2	1.2		
63.70		2	1		
64.60		1.2	2		
65.96		2.3	1.2		
68.65		3	2.3		69.3
——	4468.65				
69.32		1	1.2		
71.00		1.2	2		
71.40		2.3	1.2		
75.00		2.3	1		
79.86		2	1	⊙ line a weak companion to 79.80.	
80.72		2	1.2		
81.41		2.3	2		81.8
82.84		2	2		

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
4488.47		1.2	2		
89.24		2.3	1.2	⊙ line close double { 89.25 .35	
92.70		1.2	1	⊙ line distinct. Wanting in R.	
——	4494.72				
95.19		3	1	⊙ line distinct. Wanting in R.	
96.33		3	2		96.9
97.90		1.2	1.2		
4501.43		3	2.3		01.6
03.92		2	1		
06.51		1.2	1		
08.21		1	—		
——	4508.45				
11.32		1.2	?	perhaps a ⊙ line.	
12.88		3	2		12.4
15.76		1.2	—		
18.18		3.4	2		18.4
18.84		2	1		
22.97		3	2		22.9
27.48		3	2.3		27.1
33.42		3.4	2.3		33.0
34.15		2.3	3		
34.97		3.4	2.3	this group is in Th. a broad irresolvable band. "Très large."	
35.75		3	2.3		
36.12		3	2.3		
36.25		3	2.3		36.5
——	4536.25				
44.83		3.4	2.3		44.4
48.93		3.4	2		
49.79		3	2.3	⊙ line triple { 49.60 Fe .79 Ti .94 } K.-R.: 49.64. (Fe)	49.8
52.62		3.4	2	⊙ line close double { 52.62 Ti .72 Fe }	52.7
55.64		3	2		56.2
58.02		1.2	—		
58.28		1.2	1.2		
60.08		2	1		
62.80		2	1		
63.60		1.2	1		
——	4563.94				
63.94		2.3	2.3		64.1
71.07		1.2	—		
72.15		3	2.3		72.4
75.71		1	—		
——	4578.72				
90.11		2	2		
94.28		1	1.2		
99.40		2	1		
4609.55		1.2	—		
——	4611.44				

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
4614.47		1	—		
17.41		3.4	2		17.6
19.67		1.2	1.2		
23.24		3	2		23.9
29.47		2.3	2.3		29.9
34.87		1.2	1.2		
35.04		1.2	2		
35.71		1.2	1		
37.34		1	1		
——	4637.67				
38.04		1.2	1		
39.50		2.3	2	}	
39.83		2.3	2	}	39.7
40.11		2.3	2		
40.60		1	1		
45.36		2.3	1.2		44.9
50.16		2.3	1		
55.82		2	1		
56.20		2	1		
56.60		3.4	2		56.9
57.35		1.2	1.2		
67.76		4	2.3	⊙ line strong double { 67.60 Fe, .75 Ti } K.-R.: 67.63	67.4
——	4668.30				
68.54		1.2	?	perhaps a very weak ⊙ line.	
75.27		2.3	1.2		
——	4679.02				
82.08		3.4	2		82.4
84.68		1.2	1		
87.08		1.2	—		
87.97		1.2	—		
88.56		1.2	1		
90.97		2	—		
91.50		3	3	⊙ line very close double; not separated. Ti corresponds to the component toward violet K.-R. give 91.59=Fe.	91.5
93.83		2.3	1		
97.10		2	?		
98.94		3	1.2		98.9
——	4703.98				
4710.34		3	1	⊙ line close double { 10.34 Ti, .44 Fe }	09.8
15.46		2	1		
22.77		2.3	1.2		
23.32		2.3	1.2		23.6
——	4727.62				
31.33		2.3	1		
33.58		2	1		
34.83		1.2	1		
42.28		2	?	possibly a weak ⊙ line.	
42.94		3	1.2		42.6

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
4747.84		2	1		
——	4754.22				
58.30		3	2		57.8
59.08		1.2	1		
59.44		3	2		59.3
66.48		2	?		
69.94		2	1		
78.44		2.3	1.2		
81.91		2	1		
——	4783.60				
92.65		2.3	2		92.4
96.36		2	1.2		
98.13		2	1		98.3
99.95		2.3	1.2	⊙ close double { 4799.95 Ti / 4800.05 Fe	
——	4805.25				
4805.25		1.2	2.3	⊙ line double. Which component?	05.2
05.56		2.3	1.2		
08.70		2	1		
11.24		2	1		
12.40		1.2	—		
19.20		1.2	—		
20.56		3	1.2		20.4
——	4824.31				
25.63		1.2	1		
27.74		1.2	1		
36.25		2	1.2		35.9
41.00		3.4	2		40.9
44.13		1.2	—		
48.62		2	1		48.9
56.18		3	1.2		55.9
——	4859.93				
64.37		1.2	—	⊙ has 64.45; not coincident.	
68.44		3	1	⊙ double { 68.44 / .55	68.4
70.28		3	1.2		69.9
81.08		1.2	1	⊙ line uncertain. 73.9 (Th) wanting.	
82.53		1	—		
85.25		3.4	2		85.4
——	4890.94				
92.03		1.2	—		
93.25		1.2	—		
93.62		1	—		
4900.08		3	1.2		00.1
——	4903.48				
11.38		1.2	1.2		
13.76		3	1.2	⊙ has 13.75, 14.05.	14.0
15.40		1.2	—		
19.99		2.3	1		19.8
21.90		2.3	1.2		21.6

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
4925.53		1.2	1		
26.31		1.2	?		25.8
28.50		2.3	1.2		28.3
——	4934.24		—		
37.94		1.2			38.0
38.51		2	—	quite near a ⊙ line, but displaced somewhat toward red.	
41.77		1.2	—		
48.40		1.2	1		47.8
64.90		2 —	?		65.3
68.75		2	1		68.5
73.25		2	2		73.0
75.52		2.3	1.2		76.0
77.92		1.2	1.2		
78.39		2	1.2		78.6
——	4978.78				
81.91		4	2.3		81.8
89.33		2.3	1.2		89.1
91.24		4	2.3	⊙ has { 91.25 .45	91.1
97.26		2.3	1.2		
99.67		4	2.3		99.6
5001.16		2.3	1.2		02.0
——	5007.42				
07.42		4	3		07.6
09.81		2	1		
13.45		3	1.2		13.2
14.40		4	3		14.3
16.32		3.4	2		16.3
20.17		3.4	2		20.4
23.02		3.4	2		22.2
25.00		3	1.2		24.8
25.72		3	1.2		25.8
——	5036.10				
36.10		3.4	2.3		
36.65		3.4	2		36.6
38.55		3.4	2		39.2
40.12		3.4	2		40.2
40.78		2	1		
43.77		2	1		44.4
45.58		1.2	1		
53.06		2	1		53.3
54.30		1	—		
——	5060.25				
62.30		2	1		62.3
64.26		1.2	?		
64.82		3.4	2.3		65.4
66.12		2	1.2	⊙ line probably double.	66.5
68.47		1.2	1		
69.56		1.2	1		
71.66		2	1.2		71.2

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
5085.55		1.2	?	The line 77.5 in Th. missing.	
87.24		2.3	1		87.5
	5090.96				
5103.39		1	—		03.5
09.65		1.2	—		09.7
13.64		2.3	1+		14.1
20.60		2.3	1.2		21.0
——	5121.80				
29.32		1.2	2.3		29.7
33.12		1	—		
45.62		2.3	1.2		45.6
47.63		2.3	1.2		48.1
52.36		2.3	1.2		52.3
——	5155.94				
73.94		3	2		74.1
86.57		1.2	—		86.2
88.87		2	2.3	⊙ line double { 88.87 Ti, 89.05 Ca	89.4
93.15		3	2		93.4
94.25		1.2	1		
5201.32		1.2	1		01.6
——	5202.49				
06.30		1.2	3	⊙ line double; coincidence with the component toward red.	06.6
08.08		1.2	1		
10.55		3	2		10.6
12.50		1.2	1		
19.88		2	1	the line 18.6 (Th.) missing.	
22.87		2	1		
23.80		1.2	1	the line in Thalén is described as "large" and therefore probably includes the entire group.	
24 46		2.3	1.2		24.1
24.71		2	1		
25.15		2.3	2		
26.70		1.2	2		27.1
——	5230.01				
38.77		2	1		39.6
46.30		1	—		
46.75		1	—		
47.48		1.2	1		47.4
51.14		1	1		
52 26		2	1		52.1
56.01		2	—		56.1
60.18		1.2	?	perhaps a ⊙ line.	60.7
63.71		1.2	—		64.0
66.20		2.3	1.2		66.1
——	5266.73				
82.61		1.2	1.2	Thalén's lines 72.6, 68.3 missing.	
83.63		2.3	1		83.9
84.61		1.2	1.2		
89.02		1.2			88.9
95.95		2	1		96.6
97.42		2.3	1		97.8

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
5298.61		2	?		
5300.18		1	1		99.6
——	5300.92				
36.96		1.2	2		
38.54		1	1		38.1
41.68		1	—		
51 28		2	1		51.7
66.85		1.2	—		
——	5367.67				
69.81		2.3	1.2		70.0
81.20		1.2	2		81.4
89.36		1.2	?		
90.23		2	1		
96.78		1.2	1		
——	5397.34				
97.28		2	2.3	Fe has 97.34 (K.–R.), Ti appears to be more refrangible.	97.3
5404.25		1.2	—		04.4
09.81		2.3	1		09.9
19.00		1.2	1.2		19.2
——	5424.27				
26 48		1.2	1		26.3
29.37		2.3	—		29.9
36.93		1.2	—	sharp.	
38.53		1.2	—		
46.80		2	2		47.1
——	5447.12				
49.40		1.2	—		49.3
53.88		1.2	1		
60.72		1.2	1		
——	5466.60				
71.43		2	1		71.8
72.90		1.2	1.2		
74.43		2	1		74.6
77.92		2.3	1		77.8
81.64		2.3	2		
82.09		2.3	1		81.5
——	5487.96				
*88.44		2.3	1		88.1
*90.88		3	1		90.2
5504.10		2.3	1		04.2
12.00		1.2	?		
12.72		3	1.2		13.2
——	5513.19				
*14.58		3	1.2		
*14.78		3	1.2		14.8
65.70		3	1		66.0
——	5569.84				

*The asterisk signifies that the wave-lengths were derived from the preceding ⊙ line.

Titanium λ	R.	i Ti	i ⊙	Remarks	Thalén
				All the following lines lie within a series of flutings which begin with a sharp edge at λ 5597.90 and extend with decreasing intensity toward the red. The flutings are all resolvable into elementary lines.	
5644.37		3	1.2		44.3
48.81		2.3	1		48.3
—	5658.09				
62.37		3	1		62.8
63.16		2	2		
75.61		3.4	2		75.7
80.15		2	1	⊙ line weak; not in R.	80.3
—	5688.43				
89.70		3	1		89.8
5702.92		2.3	1		02.7
08.46		2	?		
12.07		2	2.3	sharp.	
14.12		2+	?	perhaps a ⊙ line.	
15.30		2.3	2		15.2
16.71		2	?		
20.70		2	1	sharp.	
—	5731.98				
39.69		2.3	1		39.2
40.20		1.2	1	sharp.	
—	5754.89				
57.08		1.2	1.2		
62.52		2.3	1	diffuse; in Rowland, trace of a ⊙ line; not on the plate.	
66.56		2.3	1	diffuse.	
—	5772.36				
74.27		3	1		
81.04		1.2	1		
86.21		3	1	diffuse.	
—	5791.21				
3804.45		3	1	diffuse.	
—	5805.45				
23.95		1.2	—		
—	5831.84				
66.69		3.4	2		66.5
—	5884.05				
80.55		1.2	?		
—	5893.10				
99.56		3	1.2		5900.2

8. As for the accuracy of the wave-lengths here given, the probable error will seldom exceed ±0.02 tenth-meters. In order to guard against such systematic errors as are easily produced by a slight relative displacement of the metallic and solar spec-

tra on the photograph, only those plates out of the many available ones were measured which exhibited the greatest possible sharpness of definition, and on which the always present iron lines were in exact coincidence with the corresponding lines in the solar spectrum. That the resulting wave-lengths are in fact free from such errors is shown partly by the very satisfactory agreement in every respect with the measurements of iron lines by Kayser and Runge, and with those of a few titanium lines by Rowland, and partly by the agreement of the two series of measurements of titanium lines executed on different plates. Comparisons with the iron spectrum of Kayser and Runge have already been given. If the above table is compared in like manner with Rowland's catalogue of normal wave-lengths, we obtain from the titanium lines in the latter the following short table:

Rowland	H.	Rowland	H.
3510.99	10.98	4691.58 { Ti	91.50
3564.68	.....	4691.58 { Fe	.....
3635.62	35.61	4805.25	05.25
3653.64	53.61	4900.10	00.08
3722.69	22.70	4973.27	73.25
3743.50	.....	4981.91	81.92
3924.67	24.67	4999.69	99.67
4306.07	06.07	5007.43	07.42
4391.15 { Fe	.....	5014.42	14.40
4391.15 { Ti	.....	5020.21	20.17
4501.44	01.43	5036.11	36.10
4508.46 Ti ?	.....	5064.83	64.82
4563.94	63.94	5154.24 Ti ?	.....
4578.73 Co, Ti	.....	5188.86	88.87
4590.13	90.11	5193.14	93.15
4629.51	29.47	5210.56	10.55
		5675.65	75.61

It will probably be admitted that this agreement leaves nothing further to be desired. That lines which Rowland regarded as titanium lines are not found in my table, is no doubt due to the fact that by me they have been regarded as iron lines and have therefore been excluded.

A judgment of the close agreement of the two series of measurements of titanium with one another, by which is proved the absence of systematic errors in consequence of a displacement of the spectrum either in taking the photograph or by the dis-

tortion of the gelatine film, is most easily obtained by the aid of the following table, which gives the number of cases in which the difference of wave-lengths of the two series reaches the value set opposite:

$\Delta =$	$N =$
Tenth-meters	
0.00	125
.01	203
.02	139
.03	91
.04	45
.05	30
.06	19
.07	11
.08	6
.09	3
.10	3

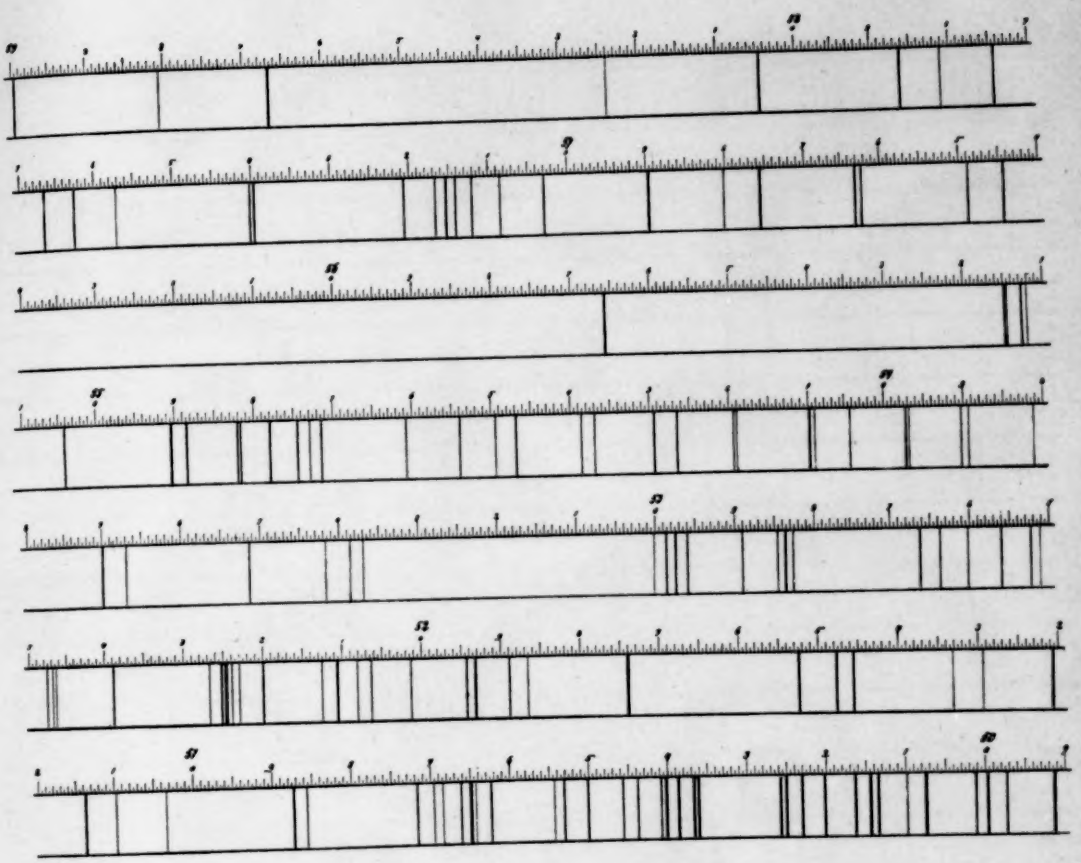
The number of cases in which the difference of wave-lengths measured on different plates exceeds 0.05 tenth-meters, or the approximate limits of the probable error, is accordingly only 42 out of the 675 lines compared, or about 6 per cent., a result which may be regarded as perfectly satisfactory.

9. The comparison of Thalén's wave-lengths with those herein given is of especial interest. If we form the differences H.-Th. for the 160 lines common to both spectra, we find that they are not only small but in general of quite a fortuitous character. We may therefore conclude that the probable error of one of the wave-lengths given by Thalén is only $\pm$ 0.24 tenth-meters, a degree of accuracy which at that time was quite unparalleled.

10. The presence of titanium in the solar atmosphere is confirmed, with even superfluous evidence, by these investigations. If we separate the coincidences and non-coincidences with the aid of columns 3 and 4, and arrange them according to the intensity of the titanium lines, the following table is obtained:

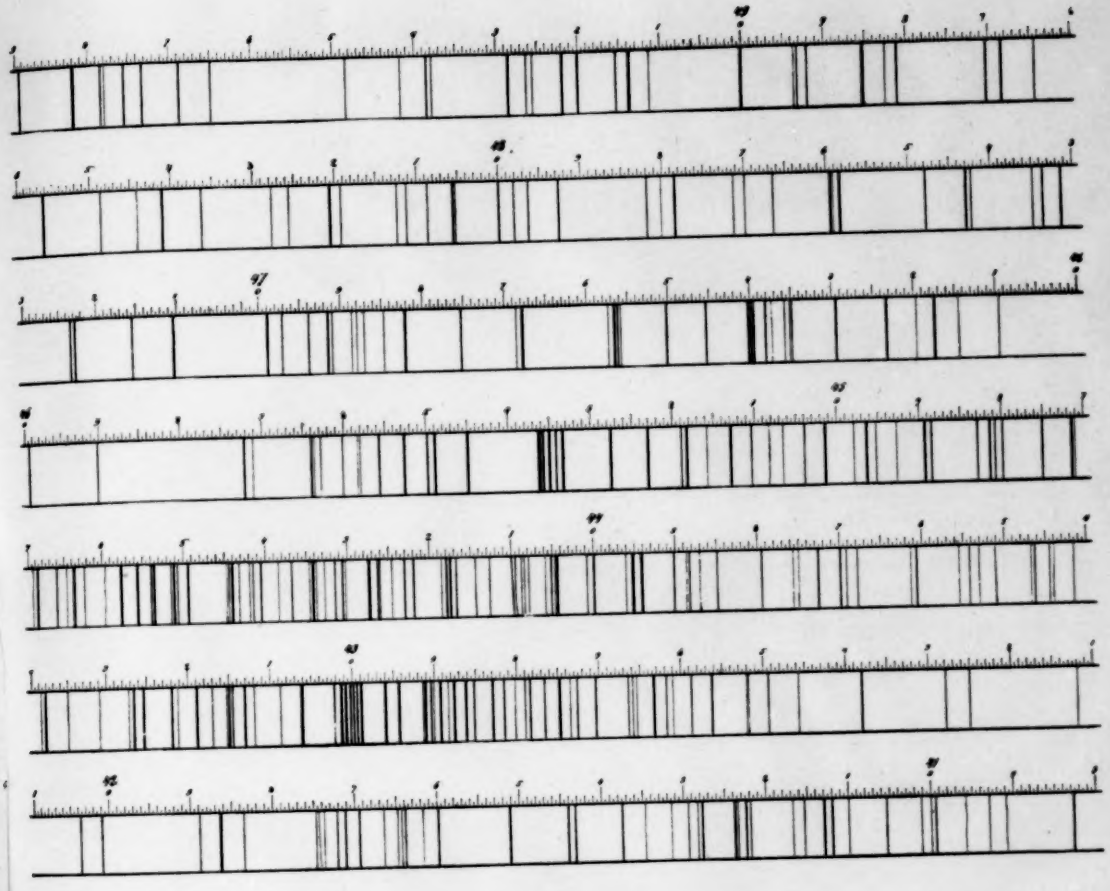
i	Coincident	Non-coincident
1—1.2	193	106
2—2.3	231	50
3—3.4	124	0
4—4.5	12	0
5—6.	2	0

PLATE IV.



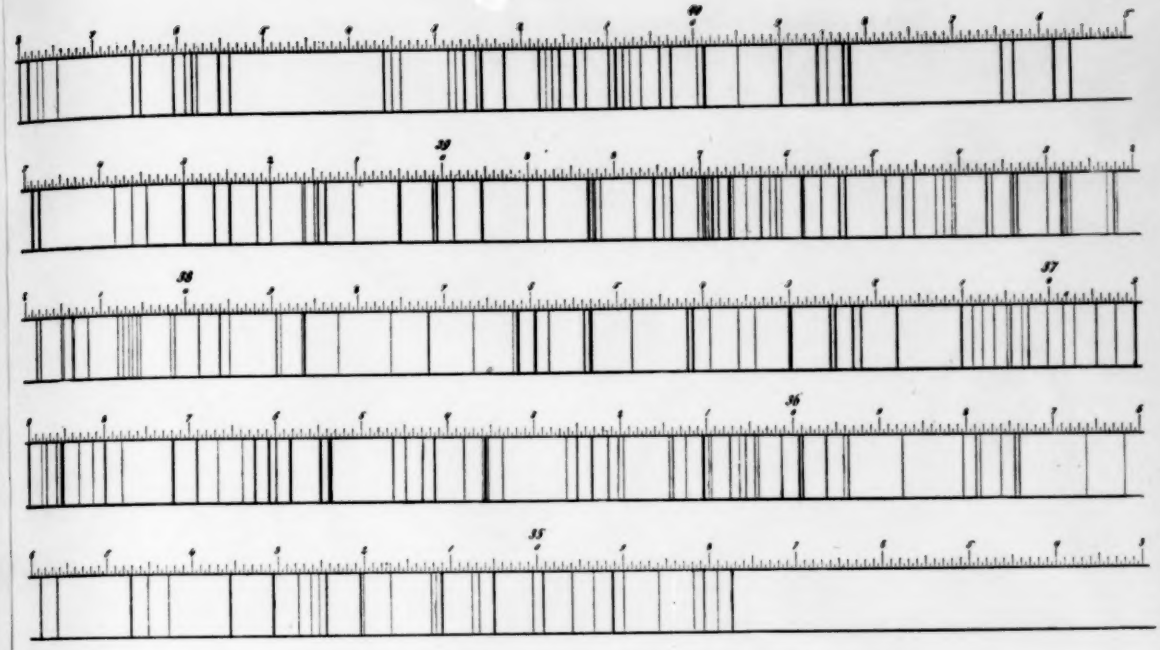
THE ARC SPECTRUM OF TITANIUM

PLATE V.



THE ARC SPECTRUM OF TITANIUM

PLATE VI.



THE ARC SPECTRUM OF TITANIUM

The coincident lines therefore form a little more than 78 per cent. of all the lines observed. If we exclude the weakest class altogether, in consideration of the probability that quite a number of the lines in it do not belong to titanium, the percentage of coincidences rises to 88. As in the case of chromium, a consideration of the intensities of the metallic and solar lines shows a general agreement, which is, however, often far from perfect.

Minor Contributions and Notes.

HARVARD COLLEGE OBSERVATORY. CIRCULAR NO. 10.

SIX NEW VARIABLE STARS.

Lists of suspected variable stars are published in the *Results of the National Argentine Observatory*, Vol. XVI, p. 32, and Vol. XVII, p. 11. These lists contain 527 and 232 stars respectively, in which the magnitudes were found to be discordant in the observations of the Cordoba *Durchmusterung*. Especial attention is there called to 26 stars, which are indicated by exclamation marks. These stars have been looked for on a number of Draper Memorial photographs by Miss E. F. Leland and the results confirmed by Mrs. Fleming. From this examination confirmation of the variability of the stars -24° 12600, -27° 15203, -33° 185, -34° 224, -38° 138, and -38° 13089 has been obtained, the change exceeding one magnitude in all cases. The variation of -22° 13401, -22° 13700, -23° 8083, -24° 13621, -25° 1197, -30° 12799, -33° 13321, -35° 11936, -35° 14568, -37° 11462, -38° 2639, and -41° 12260 on from 8 to 25 nights did not exceed two or three tenths of a magnitude, and the variation of these stars is accordingly not as yet confirmed. In each of these cases two comparison stars were selected, differing about half a magnitude in brightness, one a little brighter and the other a little fainter than the suspected variable. The star -22° 15937 does not appear on photographs taken on 16 nights, although the adjacent star, -22° 15939, is well shown on all. The confirmation by Miss Leland of the variation of -24° 7693 has already been announced (*H. C. O. Circular No. 7*). The confirmation by Mr. Robert H. West of the variation of -25° 1602, -26° 892, and -30° 375, has already been announced by him (*Ast. Jour.* **16**, 85). -25° 1602 and -30° 375 have also been confirmed here from the examination of the photographs. -30° 19092 is R Piscis Austrini. The variation of -33° 13234 was discovered independently by Mrs. Fleming (*H. C. O. Circular No. 6*). The star in the Bonn *Durchmusterung*, -22° 4346, and not found by Thome, does not appear on photographs taken on 8 nights.

It therefore appears that of these 26 stars, 12 are variable, the variability of 12 is not confirmed, and 2 do not appear on the photographs examined.

The laborious work of taking out all the photographs of the regions containing these six new variable stars, measuring the brightness, the magnitude at maximum and minimum, the period and form of light curve, as has been done for other variable stars discovered here, is now in progress.

EDWARD C. PICKERING.

AUGUST 13, 1896.

HARVARD COLLEGE OBSERVATORY, CIRCULAR NO. 11.

A NEW SPECTROSCOPIC BINARY μ^1 SCORPII.

FROM an examination of the Draper Memorial photographs Professor Solon I. Bailey has found that the star μ^1 Scorpii is a spectroscopic binary. This star is $-37^\circ 11033 = S.\,M.\,P.\ 5794$. Its approximate position for 1900 is in R. A. $16^h\ 45^m.1$, Dec. $-37^\circ\ 53'$, and its photometric magnitude is 3.26. Its spectrum is of the first type and contains the additional lines characteristic of the Orion stars. It, therefore, belongs to Class B according to the notation of the Draper Catalogue. The star μ^2 Scorpii follows about 28^s and is $1'.7$ north, and its photometric magnitude is 3.74. The spectra of both stars, therefore, appear side by side in the photographic plates. In some they are scarcely distinguishable, while in others the lines of the first star are broad and hazy, some of the faint lines appearing distinctly double. The lines in the spectrum of the second star are always single and well defined. One of the components of the binary is fainter than the other, so that the lines of its spectrum are sometimes of greater, and sometimes of shorter, wave-length than those of the brighter component. The difference in intensity also seems to change as if due to a variation in the light of one of the components.

An examination of the plates already sent to Cambridge shows that the spectrum of this star was photographed on October 2, 1892, July 20, 1894, and July 31, 1894. On the first of these plates the lines are single, on the second they are wide and hazy, and on the third they are double. When Mrs. Fleming examined these plates in October 1894, she recorded on the second of them "Lines double?" and on the third of them "Lines double." They were laid aside for further examination but were overlooked.

The changes are so rapid that they are perceptible in successive plates having exposures of one hour. From a discussion of fifty-two photographs Professor Bailey has derived a period of thirty-five hours and a nearly circular orbit.

An independent discussion made here shows that all the observations, including those of the early photographs, may be represented by the formula, *J. D.* 2412374.52 + 1.4462 *E.* which gives an average period of $34^h\ 42^m.5$ with an uncertainty of less than a tenth of a minute. Ten observed times when the lines were single are represented with an average deviation of thirty-eight minutes each. The maximum deviation is less than an hour. Further observations will, however, be required to make sure that no other assumption is possible for the total number of epochs since 1892. In nineteen photographs the lines of the fainter component were found to have greater, and in fourteen, shorter, wave-lengths than those of the brighter component. This was confirmed later, in each case, by the formula.

Only two stars of this class are already known. The first, ζ Ursae Majoris, was found by the writer in 1889. The period appears to be about fifty-two days, but it is irregular, perhaps owing to the presence of a third body and the time during which the lines are widely separated is short. The second star, β Aurigae, was found later in the same year by Miss A. C. Maury. The changes appear to be perfectly regular with a period of a little less than four days.

EDWARD C. PICKERING.

August 31, 1896.

A NEW ASTROPHYSICAL OBSERVATORY.

WE take pleasure in announcing the establishment of a new observatory at Rössgen, Mittweide, Saxony. The principal instrument, which will be ready for use by the middle of October, is a refractor of 170^{mm} aperture, made in the workshop of Hans Heele in Berlin. It is provided with both visual and photographic objectives, and the mounting embodies a number of new features. The programme of work prepared by Dr. Friedrich Krueger, the director of the observatory, includes:

(1) The formation of a photometric catalogue of all colored stars within the limits of the director's catalogue of colored stars.

(2) Photometric determinations of comparison stars used in the

observation of variables, including such stars as are communicated to the director by observers of variables and those which are found in published papers.

(3) Construction of star charts, by the aid of photography, of regions containing variables.

HUBERT A. NEWTON.

THE recent death of Professor Hubert A. Newton, of Yale University, removes one who has contributed largely to cometary and meteoric astronomy. Born in Sherburne, N. Y., March 19, 1830, of a mother locally famed for her mathematical skill, Newton gave early promise of his power as a mathematician. After preparing for Yale in the school of his native town, he entered college at the age of sixteen. His course was followed with success; among other distinctions he won the first prize in mathematics and gained some reputation as a public speaker. His study of mathematics was continued after graduation, and in 1853 he was appointed tutor at Yale University. The work of only two years brought him, at the age of twenty-five, an appointment to a full professorship in the mathematical department, where he served with distinction up to the time of his death.

After some years devoted to investigations of a purely mathematical character Professor Newton undertook, shortly after the meteoric shower of 1853, the researches for which he is best known. He was able to identify the meteors with the comet of 1866, and to predict their periodical return. The long series of papers published during the remainder of his life greatly stimulated the scientific study of meteors and their relations with comets. As Secretary to the Board of Managers of the Yale Observatory, and for two years as Director, he was identified from its inception with this well-known institution. Although his theoretical researches left him little time for observational work, he did much to encourage the important investigations of Dr. Elkin and his associates. For all of these services to science he received deserved recognition from learned societies at home and abroad. His personal character endeared him to a large circle of friends, who will unite with the many who knew him only by reputation in mourning his loss.

Reviews.

On the Equipment of the Astrophysical Observatory of the Future, with two appendices: Appendix I., *On the Support of Large Specula*. Appendix II., *On Making the Siderostat an Instrument of Precision*. By G. Johnstone Stoney, M.A., D.Sc, F.R.S. *Monthly Notices of the Royal Astronomical Society*, Vol. LVI., pp. 452–459, June, 1896.

The general design and equipment of an observatory which is to be devoted simply to the science of observational astronomy has now become well established, and indeed, from the nature of the work undertaken, admits of but little variation. But astrophysical work has now come to occupy such an important place in astronomical science as to demand observatories specially designed and equipped for its prosecution. The number of such observatories is as yet small and the general lines upon which different ones have been fitted up vary widely. Some, like the astrophysical observatory at Potsdam, have retained the equatorial, simply attaching all the necessary apparatus to the eye-end of the telescope. Others, of which the one established by Langley at Allegheny was one of the first, have adopted some form of the siderostat, usually the Foucault, in connection with fixed observing apparatus.

This being the case, Dr. Stoney's discussion of the form and equipment of astrophysical observatories is especially seasonable, although one might wish that it were more extended. The use of the equatorial, for example, is not considered at all, the author taking the ground that some form of siderostat is always to be preferred for astrophysical work. With this conclusion many astrophysicists will disagree. Dr. Stoney's objections to the equatorial are: (1) The expense of the mounting and of the movable dome. (2) The fact that the observing apparatus must be attached directly to the eye-end of the telescope and move with it, and that this necessarily precludes the use of certain instruments, of which the radio-micrometer and the recently invented apparatus of Captain Abney for producing monochromatic images, are quoted as examples. As regards the first objec-

tion, it is to be questioned whether the cost of an equatorial mounting itself will greatly exceed the cost of an efficient siderostat of the same clear aperture, for we must remember that the use of the latter involves the use of at least one optical flat from one and one-half to two times the size of the telescope objective. The cost of the dome is undoubtedly greater than the cost of the buildings necessary to contain a siderostat, especially when the telescope has a very small angular aperture and consequently great focal length. Many plans, however, have been proposed for doing away with the expensive dome construction, and it is quite possible that some arrangement quite as efficient, and, for large telescopes, far less expensive, may before long be arrived at. One arrangement which has recently suggested itself to the reviewer would be to use a short cylinder of a diameter somewhat greater than the focal length of the telescope, and of a length somewhat greater than the length of the declination axis, mounted with its own axis horizontal and passing through the point of intersection of the polar and declination axes of the telescope. This cylinder would be mounted on its lower side on a horizontal ring which surrounds the base of the telescope mounting and upon which it may revolve about a vertical diameter. The lower half of the inner cylindrical surface could be cut into steps to form a series of observing platforms for different altitudes, one-half of the upper surface being cut away to form the usual slit extending from the horizon to the zenith. In order to increase the stability in high winds it might be advisable to place a bearing at the top of the cylinder, the box of which would be secured in position by means of steel guy ropes.

The second objection to the equatorial is, so far as the actual use of astrophysical instruments is concerned, more apparent than real. There are many forms of present laboratory apparatus which could not, it is true, be attached to the eye-end of a telescope, but there is not so far as the reviewer is aware any important astrophysical work that could not be carried on with properly chosen types of instruments equatorially mounted. For example, while the radio-micrometer cannot be used in this way, the bolometer can be with great facility, and the latter instrument has some other important advantages over the former, especially in spectrum work. Similarly the Hale spectroheliographs are likely to give better results in securing monochromatic images of the Sun than the Abney form of instrument, and no difficulty

is found in adapting them to use with the equatorial; in fact, it is extremely doubtful whether they could be used with equal advantage in any other way. The equatorial has in general the great advantage of simplicity, of minimum loss of light, and of maintaining the position of the observing instrument with respect to the image constant. In accuracy of following also the simple equatorial is superior to any form of siderostat except those in which the specula are directly attached to and move with the telescope, as in the polar heliostat or the Coudé equatorial. The last three advantages are of primary importance in all photographic work in which prolonged exposures are necessary, and also in stellar spectroscopic work either visual or photographic. Dr. Stoney says, "The only defect which the siderostat has, namely, that the image furnished by it slowly rotates in the field of view, is of no detriment for stellar spectroscopy." Only those who have had actual experience in the work know how important it is to keep the slit of a stellar spectroscope parallel to the direction of the motion in right ascension. The great advance in stellar spectroscopic work will, in the opinion of the reviewer, come in the use of larger apertures of telescope and the substitution of reflectors for refractors, a substitution the many advantages of which have been already frequently enumerated by the writer.[1] In the case of very large apertures, too, the use of one or two additional specula of the same aperture as that of the telescope (as would be necessary in any form of siderostat), would very seriously increase the expense and the difficulty of mounting and guiding, additional arguments for adhering to the simple equatorial form.

One great disadvantage of this form of mounting which was not stated by Dr. Stoney and which really seems to demand more than anything else the use of a siderostat for some special classes of work, is the inability to keep the large dome, in which the equatorial is generally mounted, at a constant temperature and under constant hygrometric conditions.[2] When the observing instrument is small this difficulty may be overcome by enclosing it in a non-conducting case, or better in a series of concentric copper cylinders or boxes, but only at some sacrifice of convenience of manipulation. Besides the

[1] See particularly *Phil. Mag.*, July 1894, and Oct. 1894. *A. and A.*, Dec. 1894. *Ap. J.*, 1, 70; Jan. 1895, 1, 232; March 1895, 3, 182; and March 1896.

[2] Of special importance for work with the spectro-bolometer in which a rock-salt prism is employed.

comfort of the observer is sometimes an important factor in determining the success of the work, and this can without doubt be best secured by using some form of siderostat. Of these the reviewer agrees with Dr. Stoney in considering the polar siderostat, in general the most advantageous. In this connection it may be well to point out the great advantage which would result from reflecting the beam down the polar axis instead of up as is usually done; this advantage being that for those parts of the sky to which the instrument is most frequently directed, *i. e.*, the regions in the neighborhood of the celestial equator, the speculum is nearly vertical and therefore in a position of minimum flexure, instead of nearly horizontal, as in the usual arrangement.

Another important advantage is that in this arrangement the mirror of a polar heliostat may be mounted directly on the lower end of the polar axis of an ordinary equatorial and the one mounting, and what is perhaps even more important the one dome, thus made to serve perfectly for two independent instruments. As the tube of the polar heliostat lies in the prolongation of the polar axis of the equatorial it can never interfere with the telescope of the latter in any position of use. It is frequently very desirable to carry on two independent lines of work on the same object, particularly the Sun, simultaneously, and in such a case this arrangement offers great advantages. By clamping the mirror mounting to the telescope axis in different positions with respect to the declination axis, the two instruments may be used on different objects, but in the latter case two independent slit openings in the dome would be necessary.

The coelostat has, as Dr. Stoney points out, some very important advantages, and it deserves to be more generally used than it has been heretofore. The latter instrument ought to prove especially serviceable for spectroscopic and radiometric studies of solar details—particularly on eclipse expeditions—on account of its simplicity and the ease with which it can be set up and controlled. The author's plan of combining the polar heliostat and the coelostat is in some respects a good one, but it is open to the objection that the two instruments cannot be used together even if the mirrors are on opposite ends of the polar axis, owing to the different rate of rotation necessary. For this reason the combination of the polar heliostat and the equatorial as suggested above would seem more economical and efficient.

The merits and advantages of the s'Gravesande, or, as it is com-

monly known, the Foucault type of siderostat are, in the opinion of the reviewer, generally overrated. The beauty of the geometrical solution and the fascination of the idea of being able to maintain the telescope and all its connected observing apparatus fixed and immovable in a horizontal plane, in a room furnished with all the conveniences of a laboratory, is apt to blind one to the inherent difficulties of the mechanical design ; one of the greatest of these being the extremely unfavorable angle at which the driving mechanism acts when the object in the field is more than 50° from the zenith.

It is pretty safe to say that these difficulties will always prevent the use of this instrument for work requiring extreme precision and steadiness of image. The performance of this instrument can no doubt be greatly improved by careful attention to mechanical details, particularly those having to do with the driving connection between the equatorial axis and the mirror support. The rectilinear motion described by Dr. Stoney in Appendix II. will, if carefully made, no doubt work better than the sliding joint usually employed in making this connection, but in the form shown it is cumbrous and lacks the rigidity that is desirable at this point. A modified form of the Hart or Peaucellier straight line motion, or a well made ball-bearing attached to the end of the declination axis and arranged to allow of both longitudinal and circumferential motion on the driving rod attached to the back of the mirror, would probably work better in actual practice.

The form of mirror support described in Appendix I. is most ingenious and ought, so far as preventing flexure of the mirror is concerned, to act admirably. A mounting designed on exactly the same principle as that made use of in Dr. Stoney's arrangement, was devised several years ago by Mr. Ritchey, the optician of the Yerkes Observatory, but no description of it was published. More recently the same plan independently suggested itself to the writer of this review. In the latter case, as well as in the form finally adopted by Mr. Ritchey, it was proposed to make the inner ends of the levers bear directly against the glass, the outer ends being weighted so as to just support the weight of that portion of the glass resting on the inner end. Three fixed points on the back and three fixed points on the edge kept the mirror in an invariable position with respect to its mounting. The use of the air cushion has some advantages, but it has the objectionable feature of bringing the back of

the mirror into contact with a considerable surface of metal. In order to perform best under varying temperature conditions both the back and edge of the mirror should be as fully exposed as possible,[1] and this condition is best secured by supporting the mirror on a number of rounded, non-conducting pins of wood or hard rubber, raised some distance above the ends of the levers or part of the mounting in which they are inserted.

But it must be remembered that the question of the performance of large mirrors in different positions is not by any means a question of the mounting alone. Much of the trouble experienced with specula of even moderate size is undoubtedly due to a want of homogeneity in the glass and insufficient thickness of the latter. The excellent performance of the Coudé Equatorials, in which two optical flats are employed, one of which assumes nearly every possible position, is attributed by M. Loewy to the relatively great thickness of the mirrors in comparison to their diameter. He considers that this ratio should not be less than one-fifth or one-sixth even for the very largest specula, a much larger ratio than has been usually employed. If discs of this thickness were employed and care were taken to have the glass as homogeneous and as thoroughly annealed as that used for refractors (freedom from striæ and bubbles is not so important), great improvement in the performance of reflectors would undoubtedly result; we might indeed then hope to rival the performance of the giant refractors which have recently been constructed.

F. L. O. WADSWORTH.

[1] Large mirrors ought to be polished on the back and edge as carefully as on the front surface. They should then be silvered *all over*. Neglect of these precautions will invariably cause the mirrors to warp more or less when the temperature of the surrounding air changes.

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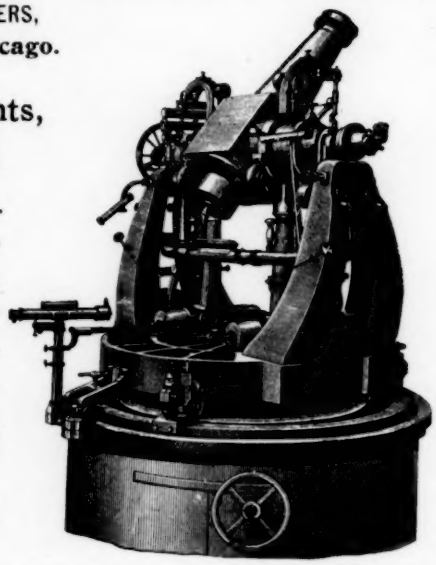

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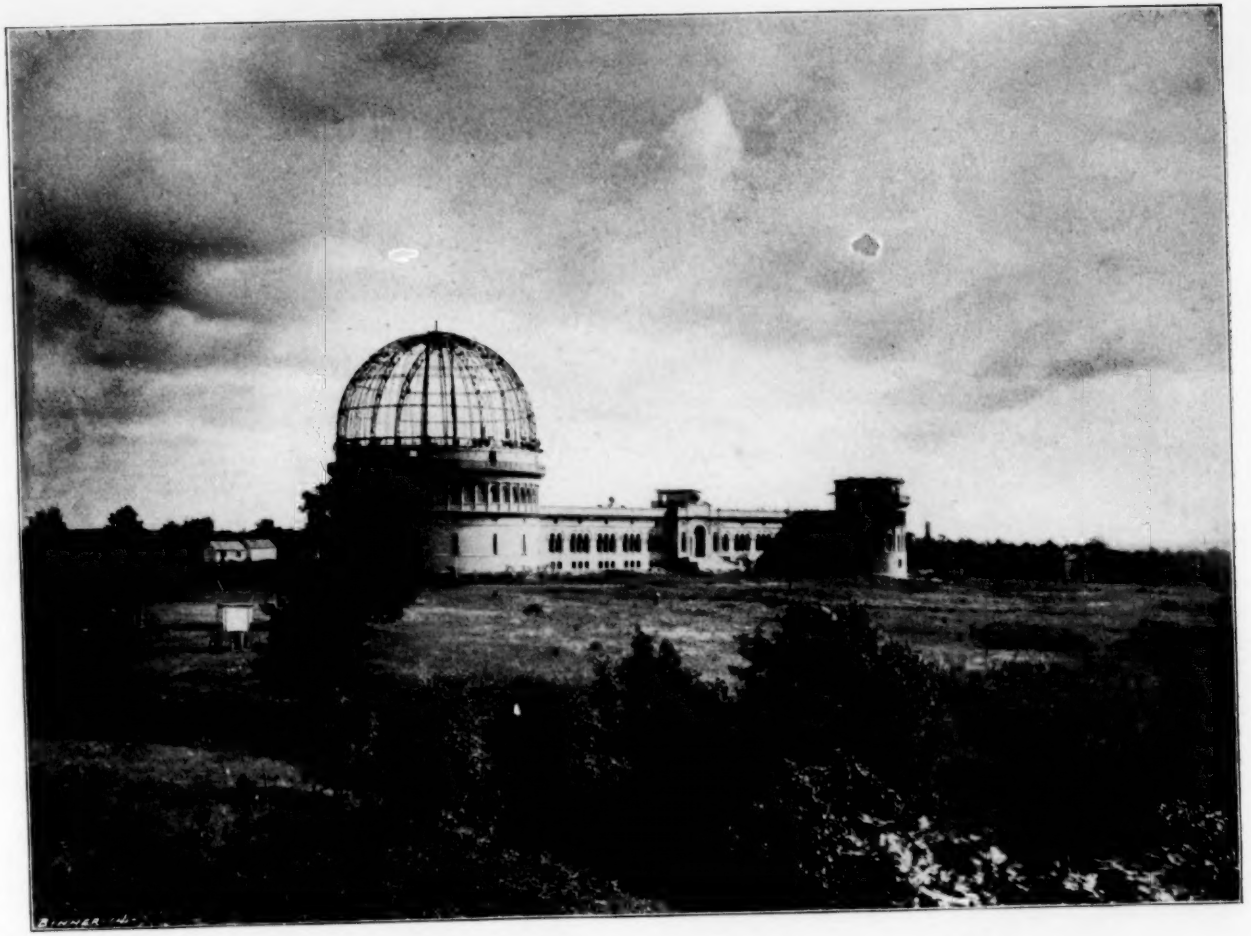

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